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CONTENTS

The Present Educational System and the Training of Economic Entomologists: DR. F. M. WEBSTER 557

On the Improvement of Medical Teaching: PROFESSOR C. M. JACKSON 566

Scientific Notes and News 571

University and Educational News 573

Discussion and Correspondence:—

Are Horns in Sheep a Sex-limited Character? PROFESSOR W. E. CASTLE. *The Moth of the Cotton Worm:* DR. LEONARD HASEMAN. *Crystallographic Tables:* DR. OLIVER BOWLES. *House Air:* G. L. MANNING 574

Quotations:—

Most Recent Investigations on the Determination, Preservative Action and Admissibility of the Use of Benzoic Acid: K. B. LEHMAN 577

Scientific Books:—

Steuer on Planktonkunde: PROFESSOR CHARLES ATWOOD KOFOID. *Ward's The Realm of Ends or Pluralism and Theism:* PROFESSOR R. M. WENLEY 585

Botanical Notes:—

The Passing of the Slime Moulds; The Secret of the Blueberries; The Grama Grasses; Botanical Notes: PROFESSOR CHARLES E. BESSEY 589

The Relation of Pigmentation to Temperature in Deep-sea Animals: CHAS. E. WOODRUFF 591

Physiological Sex Determination: BENJ. C. GRUENBERG 593

Societies and Academies:—

The American Mathematical Society: PROFESSOR F. N. COLE. *New York Section of the American Chemical Society:* DR. C. M. JOYCE. *The Section of Biology of the New York Academy of Sciences:* DR. WILLIAM K. GREGORY 594

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THE PRESENT EDUCATIONAL SYSTEM AND THE TRAINING OF ECONOMIC ENTOMOLOGISTS[1]

AT the beginning, I wish to call your attention to the fact that this paper deals with a system and its relation to the preparation of a class of men to meet certain insistent economic conditions brought about by a twentieth-century civilization. I do not wish to place the instructor in any other position than that of a more or less willing or unwilling agent of this system, or the university graduate in any other light than that of the inevitable result of the workings of that system.

The world of to-day demands the carrying out of certain undertakings, the doing of certain difficult things, which, in turn, demands certain qualifications in the men who are to do them, and demands them as never before in the world's history. These men must be taken as lads and so trained as to meet these imperative demands. Where can the American boy get this preparation that these existing conditions demand? Who has the ability, willingness and the freedom to train him therefor? If I were the only one asking these questions, or were the science of entomology alone involved, there would be grounds for suspecting that there was something wrong with the entomologists. But this is not the situation.

Several years ago Mr. F. W. Taylor, who is not only not a man with a grouch, but one of the foremost engineers in the country, stated that he had long ago made up

[1] Read before the Washington, D. C., meeting of the Entomological Society of America, December 27, 1911.

his mind not to employ another college graduate unless the latter had either worked outside in a shop for two years or else had been forced out of school and into practical work. Not only this, but Mr. Taylor goes on to say that the overwhelming majority of employers of this country want nothing to do with college graduates, if they can help themselves, until after they have been tried out, so to speak, outside the university.

More recently Mr. R. T. Crane,[2] a Chicago manufacturer, of nearly sixty years' experience with, as he tells us, all classes of men, is even more dissatisfied with the output of our colleges and universities and is much inclined to, unjustly, throw overboard all technical training, although evidently fully recognizing the importance of the practical application of science.

The self-educated or self-made man must, almost of necessity, possess an iron will, preferring to override obstacles that obstruct his way, rather than to evade them. He is, on the whole, more liable to become dogmatic, thus going to extremes, and to permit prejudices to obscure reason, than is his fellow with a technical training.

Railway people who are on the watch for men of action tell me that they like the university man with the technical training, provided he will have devoted his vacations to their temporary employment, thus carrying the theoretical and practical features along together.

The foremost electrical concern in this country, the Westinghouse, also likes to get college and university men, but is mighty careful not to let them get where they can do any damage, until they have given them a training in their own practical school.

Now it is not to be supposed that the instructor in entomology will be given advantage over his colleague, the instructor in engineering, or that his students will be allowed to follow a special educational system of his own. Indeed, he is lucky if he is able to secure even equal privileges and facilities for carrying on his work. It can not, therefore, be supposed that graduates in entomology will be any better qualified than those in other courses, and I am sorry to say they are not.

During the last twenty years, including the present, I have had with me between forty and fifty assistants, nearly all college and university graduates, and I tell you candidly I would not take another one fresh from school and with no further training, were I not obliged to do so. Indeed this is true in general. All like college and university trained men, but after they have been made over elsewhere. Now, what is the matter?

The most of those who gave university instruction to the men who afterwards worked under me are here before me, and I fully believe that, had I been placed in your position, I probably could not have done better, perhaps not as well. Besides, the university graduate is not suffering from too little instruction, but from too much of it. He has had anywhere from fourteen to eighteen years of continuous, miscellaneous instruction and instead of his mind being clear and penetrating, it is more apt to be clouded and befuddled. As Bacon puts it, "The education of the senses neglected, all after education partakes of a drowsiness, a haziness, an insufficiency which it is impossible to cure." The man fresh from the university classroom is overloaded, not with what he needs but with that which he does not require; he is afflicted with dyspepsia of the brain, or mental indigestion, and until he gets rid of these ailments he is of little use. This is why there is so much aversion to employing them in practical work or investigations.

[2] Lately deceased.

It requires, as Mr. Taylor has stated, two years of actual outside work before they can be at all profitably employed.

Now, I do not believe that such conditions as these are necessary to a college or university education, but that the cause therefor will be found in the methods employed. If we go back to medieval times and for a considerable period thereafter, we find but scant literature, and that little accessible only to the few. Therefore instruction was of necessity oral and knowledge obtained only at great cost was retained only by committing to memory with the utmost exactness. Thus was an educational system born of necessity and fitted to the conditions existing at that time. A pupil must not only receive instruction exactly as imparted, but must be able to show by examination that he could repeat what he had heard with the same minute exactness, otherwise what he had obtained would be erroneous and, therefore, of no value.

Let us now come back from medieval times to the twentieth century. To an age when ships disabled at sea may make their situation and location known a thousand miles away and receive aid from other vessels within reach, though not within sight; to an age when you can, within a few hours, communicate with your friend, whether he happens to be in Tasmania, Alaska, Capetown or Hong Kong; to an age when, if a married man, you can, seated in your hotel here in Washington, bid your wife good-night, whether she be in Boston, Chicago, St. Louis or New Orleans; when you can board a floating palace in New York and within five days step out of it in England; and when you enter a railway coach in Chicago and in three or four days step out of it in either Portland, Oregon, San Francisco or Los Angeles; or, you can leave Chicago in the same way and in eighteen hours walk the streets of New York City.

From a time when a serious question arose in the minds of men as to whether or not insects were the results of spontaneous generation, to a time when insects from all quarters of the earth are being transported about from one country to another and reared up in myriads to destroy other insects. Within the memory of some of us, the entomologists of America could be told off on the fingers of one hand, and these were engaged in describing genera and species in a way that to-day is in many instances practically unintelligible. Some years ago, I tried to get, from a man who had grown up from boyhood with Dr. Asa Fitch, some information regarding this one of the fathers of economic entomology. What I did succeed in learning was this: "Fitch was a queer fellow, always prying into things that the Almighty never intended us to know."

Thirty years ago a state entomologist in the middle west resigned his office, telling his friends in confidence that economic entomology had reached its limit, and, so far as he could discern, there would soon be nothing for the entomologist to do. At the close of A.D. 1911 the question is to get men properly trained to carry on the work that state and nation demands to be done. There is hardly a civilized people on the face of the earth among whom there are not to be found officials who are entomologists. Not a year passes by during which some of these may not be seen here in Washington, studying our methods of work, our collections, especially the former. One of America's foremost philanthropists is sufficiently aware of the importance of this to supply the necessary funds to enable these men from other countries to come and see for themselves how the science of entomology is being profitably applied in this

country. Now, these gentlemen from abroad have access to all of our publications; can read of what we do and how we do it. They could probably read up and go before an examiner and get an excellent grading on their replies, yet it is significant that a practical business man should think this insufficient, so much so that he willingly pays the expenses of such men to enable them to get into closer touch and see for themselves how investigations are carried on, the facilities required and the methods employed in their use.

There is another phase of our present-day entomology that, in passing, I wish to note, and that is the great desire among systematists for the examination of types, the desire to see and handle the precise insect that Say, Harris, LeConte, Grote and others, both at home and abroad, had before them when they described a species and gave it a technical name. Men will travel hundreds of miles, and visit foreign lands in order to do this. Now, all of the descriptions of these are in print, all can be committed to memory and a man with ordinary intelligence could go into a classroom and pass a most excellent if not indeed a perfect examination, all that you could possibly require of a student. Does not this of itself show clearly that, whether it be a university student of twenty or thereabout, or an independent student, official or amateur, of thrice that age, he must study the things themselves? You can not make up-to-date entomologists within the walls of a classroom.

Dropping, for the time being, the subject of present conditions in the matter of progress, let us take up the educational system under which the men who are yet to come into action are to be educated.

Coming from an era when oral instruction and memorizing were imperative, through an era when the educated were the priest, the astrologer, the alchemist and the philosopher, with a few of the nobility, the remainder being illiterate and therefore ignorant and brutal, when that now powerful educator, the public press, was as yet unknown, and the almost equally powerful educator, the public library, was inaccessible to all but the few, this system has come down to our radically different twentieth-century civilization, unfortunately, deplorably intact. Indeed, what was once a necessity has now become sadly perverted. Once, the student must of necessity listen and memorize with exactness what was told him, and pass a most rigid examination. Now, however, the object appears to be to get the student to remember long enough to pass his examination. It is of such that Huxley says: "They work to pass, not to know; and outraged science takes her revenge. They do pass, and they *don't* know." Count Leo Tolstoi years ago wrote that he had become convinced that written or verbal examinations were a relic of medieval scholastic superstition, and that in the present order of things, they are decidedly impossible and only harmful. And, again, Huxley says, in his twenty years' experience as an examiner, from boys and girls of elementary schools to candidates for honors and fellowships, that it was a clear case of familiarity breeding contempt, and his admiration did not wax warmer as he saw more and more of its workings. In his opinion, examination is a good servant but a bad master, and he expressed the fear that it would sooner or later come to be the master.

But, you will ask, what has all of this to do with the entomologist, whom you have hardly mentioned? Practically nothing, gentlemen, for the reason that from the age of six years, when he entered the primary school, until he is eighteen or twenty,

or half way through his university course, he is not supposed to have any idea of what his future is to be—what he is to do in this busy world. And his education has been given with no reference to whether he is to be a doctor, lawyer, engineer or entomologist. He has simply drifted with and been one in the procession. But between his senior year in high school and his entering into the university he must pass an examination. I have just shown the value of this and all other similar examinations in forming an estimate of what he really knows. In Washington, and possibly, though rarely, elsewhere, a pupil is allowed to specialize in the high school. Here in the city we have a technical high school and a business high school which, although they may not be all that is to be desired among even those most responsible for their existence, constitute a movement in the right direction and should be encouraged both here and elsewhere.

When, therefore, on coming from the high school and presenting himself before the university for admission, where for the first time he is offered separate courses from among which he must make a selection, he naturally expects that his efforts will, or ought, to be directed along a line with what is to be his future occupation; what he is to do in future. But he soon finds that while he may select his course, about the same job lot of instruction will be administered as before.

If he has come to the university of his own accord, as is probably the case, he is like a vessel with steam up, ready to put out to sea. It does not need additional force, what it requires is direction—a pilot with a firm hand and who understands his business. Instead of this, however, the applicant will in most, though not all, cases find himself at once placed in a mental strait jacket, and if it does not fit him the screws will be applied until he fits the jacket. Incisions will be made in his brain and a heterogeneous mass of instruction forced in. If he can stand the strait jacket and hold the instruction in long enough to pass his examinations, he will be allowed to continue and finally be graduated with much ado and a great flourish of trumpets. If he can not withstand this treatment he is sent home in disgrace.

Mr. Taylor tells us that after he had given up all hope of being able to use as engineers the men who had graduated and gone out from the university, but had been obliged to throw them all overboard until they had spent a couple of years outside in practical work, he found that there was a class going out from these institutions that it paid to look over with a view of securing material for successful engineers; and this was the class that honestly flunked and went back home to work. This, to use a western mining expression, was found to be "pay dirt."

As a matter of fact the embryo entomologist, on his first admission into the freshman class, or as soon as he has decided upon such a course, should be informed that he is there to work; to himself do things and that while there will be examinations he is there to study the science of entomology and not the science of passing examinations. In order to enable him to meet these requirements he should be given first that for which there is the greatest need, and thereafter he should be provided with what he needs, when, and not before, his progress requires it, precisely as with any other tool or instrument.

His first requisites will be honesty and good judgment. If he does not already possess these, he had best stay out of entomology, or any other science, for that matter. His mission, if he succeeds, will be to penetrate the unknown and tell his col-

leagues and the world precisely what he has seen. If he can not do this with strict honesty and good judgment his efforts will be worse than useless. Just the extent to which the present system of recitations and examinations tends to develop honesty and common sense I, for one, have never been able to clearly determine.

Having gained admission and taken up his freshman work, his first need will be that of language. As an investigator he will be required to penetrate the unknown and tell in clear unmistakable language what he saw there. In my own division of the Bureau of Entomology there are, or have been, over twenty-five individuals, engaged in certain investigations, whose field notes, including name of observer, locality and date, are finally copied on library cards and filed in the office under the name of the insect to which they refer. No one can tell when the information contained in these records may be required for use, who will make use of it, or where the original observer will be at that time. He may be one, two or three thousand miles away, even in another part of the world. Therefore, his records must be clear, concise, including every detail, but excluding every superfluous word, in order that any other of the division may be able to understand precisely what the observer intended to state. While the capacity for doing all of this should be attained in the freshman year, as a matter of fact if you find more than one out of every ten or fifteen university graduates who can do this you may consider that you have found an exceptional group. This seems to be one of the requisites, not obtainable in a university course, but which must be secured later.

If the student has not already had a couple of years of Latin in the high school he might take it up at once, for the reason that it contains the key to nearly one half of the English language, while German holds the key to almost all of the other half. Both German and French are especially rich in scientific literature to which he must have easy and intelligent access. In his case, it is not a classical but a practical working knowledge of these languages that is demanded, and if any of these languages are eliminated it should be Latin. With him it is not a text-book, but a business acquaintance with languages, that he requires. He will very early become aware that the German and French scientific literature with which he will come in contact and which he must read in the original and comprehend, is quite different from that of the classroom. He will be obliged to purchase dictionaries and do a lot of self-education that will not be recognized in ordinary examinations. This need not discourage him, however, as, probably, his instructor would have to do the same thing. A professor of Greek in a theological university once told me, on returning from a summer's sojourn in Greece, that he got on very well after he had become familiar with the language. Also, at the outset the student should have elementary entomology, but it should come in the way of elementary zoology with especial reference to insects. Just what text-book is to be used here I shall leave to the instructor in entomology. Those who write text-books in zoology usually know little of insects, and it is perhaps as well that they do not give more about them. The student will need to refer to considerable literature any way, and the library is the best place to be found for keeping text-books; the more they are kept and used there the better. Entomology should be studied during the warm seasons of the year when insects are alive and active, but somehow it happens that when insects are alive the student who should study them in that condition is too

deeply engrossed in the study of dead languages. This leaves only short periods in fall and spring during which it is possible to observe living insects in the woods and fields. These brief periods should be employed to the best advantage and the student should be told, at the end of the first week of the opening term, that a large quantity of material will be required for laboratory work during winter; that the institution does not regularly supply this except for reference purposes; that it is to be found in plenty in fields and woods, where he must himself procure and preserve it for future use. He should be informed that, right then and there, the system of grading, of merits and demerits, that will be in vogue during his university career will be put into operation, and he should be marked on the quantity, condition and proper preservation of this material. The instructor should by all means be able to spend one or two afternoons each week during good weather in the fields and woods with his students. This will encourage and develop their habits of observation and enable them not only to see things, but learn where and how to find insects and properly preserve them. This last faculty appears to be another factor in the education of an entomologist that frequently there has not, in his four years' training, been time to look after. Probably any one who saw the condition of material sent to the bureau or National Museum for determination would get the same impression. As a matter of fact, all of this should be required in the freshman year and the student marked on the quality of his work precisely as if done in the laboratory. He will and indeed must learn by experience that imperfect, dirty and improperly preserved insects are unsatisfactory subjects for study. It is said that an entomological instructor, in a fit of desperation, sent an unusually dull student to the college collection to find out the number of legs an insect possessed. The poor fellow, after being out for a considerable time, returned and reported that most of them were possessed of from one to five legs, but occasionally one was found with as many as six legs.

Laboratory work means so much and is so far reaching that it is impossible to overdo it. It is here that the instructor gets the closest to his students and all work together almost like colleagues, thus giving him the opportunity of his life to study his pupils and correct their individual defects. Then, there are so many things that enter into laboratory work, on which the instructor can grade students with absolute justice, developing the traits that we who are to use them later on are so anxious to see in them. I would rather have a few days with a man in the laboratory or field than copies of any number or all of his examination papers.

He should be required to make full notes which may be graded both from an entomological and a literary point of view; he should make drawings, both free-hand and otherwise, dissections and descriptions. Besides an endless variety of rearings, breedings and interbreedings can be carried out.

The aim throughout the entire university course should be to eliminate teaching or instruction just as much as possible, and substitute therefor direction, encouragement with insistence upon accuracy and completeness. The only help given a student should be that which affords the greatest aid in enabling him to help himself. Make him work and do it properly. You who are training entomologists are already doing altogether too much teaching. Fully one third of the energy of the university instructor is being thrown away on account

of being misdirected. Not only does the student fail to profit by this, but it is detrimental instead of beneficial.

Just here I wish to interject an innovation which, if carried out, would mean a great deal, not only for the student himself, but for the one into whose hands he will fall after graduation.

Let the student spend all or as much as possible of his vacations in actual field work under competent entomologists. This work should count in credits for graduation and should be based upon efficiency and quality of work done. At present, he must report for duty at his own expense, often long distances away, which, with the limited compensation he could command, would not be profitable. But if his work counted for credits in the university, he might well afford the expense involved.

In his sophomore and junior years the student will require a knowledge of botany, sufficient to enable him to recognize plants and assign them to their proper family, or genera if common. Beyond this, an entomologist had better consult a botanist. He will need some knowledge of chemistry, geology, climatology, physics, comparative zoology, comparative anatomy, morphology and physiology. He must have some training in histology. He need waste no time on higher mathematics. He can get a working knowledge of mensuration, should it become necessary to apply it. He is already being trained in a science having no superior in developing exactness, acumen, and what a knowledge of higher mathematics does not always presuppose, good judgment. Incidentally, let him read Sherlock Holmes carefully and intelligently.

A few years ago a university that could hardly be convicted of underestimating its own greatness, graduated a student in entomology. By dint of hard, practical work outside during vacation, and in the laboratory, he was fortunately better equipped in entomology than are most graduates; it was not so much a case of graduating first and completing his education afterwards. His trigonometry, however, proved almost too much for him and remained hovering over his head, like the raven of Poe, until the very last, and, even then came very nearly preventing his graduation. He is making good since leaving the university, and has succeeded in forgetting his trigonometry, which cost him so much in both time and energy, but for which he has no more use than he has for navigation; but is still handicapped for the training in histology for which, while in school, he had begged, but was prohibited from obtaining.

In languages, classroom language may well end here, probably with the freshman year, but his training in the contact, absorption, every-day-use kind, will never end. I might say here that the lazy or don't-care fellow has probably found the work uncongenial and dropped out, also during the freshman year. All of the time continue to give laboratory work whenever possible, and outside work, too, if it can possibly be arranged.

At the beginning of the senior year give your student one or more pieces of original work, no matter what so it involves and brings into action all that he has previously acquired; and include embryology, parthenogenesis, polyembryony, or any others belonging to the most advanced entomological work being carried on elsewhere. Direct him, advise him, encourage him, but make him work out his own salvation and learn to take care of himself under any condition or situation. This sort of a man will need no petting or unusual advantages. He wants only a fair chance and a square deal. He will possess a certain species of solid independence that is quite the reverse

of snobbishness. He will be completely educated before graduation instead of only in part, the remainder to come afterwards. His mind will be clear and active like the spring rushing out of the rock, clear as a crystal, unlike that of a mud puddle just after a summer's thunder storm.

Instead of taking such a man as an experiment, at $1,000 per year salary, without much of an idea of what I am really getting, I would be only too glad to recommend his appointment at $1,500. In the first instance, if sent perhaps a thousand miles or so away, he will expect to be told just what to do and how to do it. In the second case, he will keep both mail and wires busy piling up letters and telegrams telling me what he is doing and how he does it. The one must be bolstered up, the other can not be kept down. Your senior needs no teaching or instruction. He does require judicious, kindly but firm direction and you have almost made his future.

Some things are, however, amusing, even in serious matters. Quite possibly in case of the $1,000 man, his college paper may contain something like this: "Mr. A. B. C., '10, has just accepted a responsible position with the United States Department of Agriculture at Washington. University of X. Y. Z. men are much in demand by this great nation, and this demand appears to exceed the supply." When similar information is allowed escape from the president's office, even with an eye to increased appropriations, the effect is doubly demoralizing.

Now, as I have already explained, the amount of teaching and instruction required will be vastly reduced, but the effectiveness will be correspondingly increased. You will say, however, that I am dealing with only one phase of the problem, that of training the investigator, and have lost sight of the systematist and teacher.

Relative to the first, let me tell you that the systematist must in future become more and more of an investigator, and, moreover, of precisely the sort I am urging you to develop. Our present system of insect classification is all well enough if you put it away and keep it in Schmitt boxes. The moment you remove it and attempt to build a biological structure upon it, it breaks down. It is too frail and loosely put together. Let me illustrate. Some years ago we had a parasite of a very destructive aphid down in our books as *Lysiphlebus tritici*. In carrying out our investigation it became necessary to find out whether this parasite had more than a single host insect, and whether it could develop in more than one species of aphid. To this end, recently emerged males and females were allowed to pair, after which the female oviposited in several species of aphids. Both parents were then killed and preserved and all of their progeny not used in further experiments were also preserved, and thus entire broods or families were kept together. In this way females were reared out of one host species and allowed to oviposit in others until often after several hosts had been employed it would be bred back into the species whence it first originated. In all cases the host was reared from the moment of birth, while with the parasite both parents and offspring were preserved and kept together. The result of this little fragment of work was to send two genera and fourteen species to the cemetery—you may call it Mt. Synonym Cemetery if you choose—while the insect involved is now *Aphidius testaceipes*. The systematist who studies only dried corpses will soon be out of date.

Now as to the teacher: there has of late grown up among universities and colleges

a traffic, of which you will find no record in commercial literature—the training of teachers and exchanging with each other—a commerce that is indicated in university and scientific publications. This is a kind of intellectual in-and-in breeding. Among animals and plants this is only practised where the intention is to establish a fixed type that will not change but remain stable. Hence it is that your type of teacher and teaching is so fixed and to a certain degree inflexible. A teacher instructs as he was himself taught to do—taught that it was the one and only way. If he departs therefrom to the extent of substituting direction for instruction he feels as though he was a discordant note, and has somehow done something not just right.

This is not intended so much as a criticism as pointing out the fact that it simplifies correction. The matter is one that the pedagogue can change and correct, because the old method has become antiquated. The time was when the pedagogue could and did decide the sort of training necessary, and perhaps it was, at that time, best that he did so, but times have changed since then. Up to not so many years ago when instruction was even more functional than now it was hard to find a university-trained man in the employ of industrial firms or corporations. Now there are many. There must, however, be still further modifications to meet the still more exacting demands at present made upon university-trained men. The business man succeeds by being the first to see a demand as well as the first to supply it. If you will allow me to put it in a homely way, the pedagogue must get down from his antiquated pedestal that is badly affected both by dry rot and Lyctidæ and get on to another, more substantial, of concrete and steel perhaps.

You can not suppose, for a moment, that we can carry on investigations that, ten years ago, we would not have dared to touch, with men trained just as they were ten or twenty years ago. This is far from being a personal matter. It is our irresistible, progressive civilization, the pride of every American, that calls for improvement, and it must come. Slowly, perhaps, but surely.

It has not been my aim in this paper to unduly criticize the progressive instructor, or the institution that is doing its best to break away from the old régime, but to encourage and if possible aid both in their laudable efforts.

If I have been able to put into the hands of those who are to train our entomologists for us in future facts or arguments that will aid them to push for a more rational, natural and therefore easier, though none the less thorough and severe, university training, I shall have certainly accomplished all that was intended.

F. M. Webster
U. S. Department of Agriculture

On the Improvement of Medical Teaching[1]

The watchword of the present is conservation. Especially in the industrial world it has been shown that great improvement is possible by the elimination of needless waste of time and energy. Though not yet so clearly recognized, this is equally true in the field of education. Teachers, especially those in the higher institutions of learning, are notoriously neglectful of the principles and technique of their profession. Unquestionably this results in great losses due to inefficient methods of teaching. These losses, in medical education, may be conservatively estimated at twenty to twenty-five per cent. In other words, the adoption of more efficient methods of teaching would probably enable

[1] Read at the twenty-second annual meeting of the Association of American Medical Colleges, Chicago, February 28, 1912.

us to gain the equivalent of a whole year within the time now devoted to the four years' curriculum. Is not the possibility of such a tremendous saving well worth our serious consideration?

At the outset, it should be clearly understood that *uniform* methods of teaching are neither necessary nor desirable. The methods in detail must be determined by the individual teacher to meet best the varying local conditions. But equally true is the important fact that all efficient methods of teaching must be based upon well-known and well-established principles of pedagogy. Efficient teaching requires three essential conditions: (1) Complete mastery of the subject-matter on the part of the teacher; (2) a clear notion of the aim of the teaching, and (3) well-chosen methods of accomplishing the aim. The first and most essential condition, that the teacher must be a master of his subject, is everywhere clearly recognized, and will not be discussed in the present paper. The second and third conditions are those oftenest overlooked, and it is therefore necessary to emphasize certain fundamental principles of aim and methods, the neglect of which is largely responsible for inefficient teaching.

In the first place, let us therefore consider the aim of medical education. In this all will probably agree that the *primary* aim of medical education should be to train efficient practitioners.[2] And it may furthermore be taken for granted that an efficient practitioner is one who is able to observe accurately, to think clearly and to act wisely in his medical practise.

Keeping in view this primary aim and ultimate purpose of medical education, to train efficient practitioners, we may next consider the methods, the ways and means, whereby this aim is to be accomplished. If the end is to be reached most directly, if the student is to be trained most economically for the greatest efficiency, it is evident that the instruction must be adapted to his nature and learning capacity. How shall this be done? Let us see whether there is any rational principle to guide us in adapting our methods to the nature of the student. Upon this question an important light is thrown by the history of education.

A century ago, practically all teaching was based upon the doctrine of authority. It was the function of the teacher to *tell* the student what he should know and do. It was the duty of the student to be a passive recipient, to follow faithfully the precepts of the teacher. This doctrine of authority, however, was found inefficient and has long since been abandoned in rational education. It is now generally recognized that all education really worth while is based upon *self-activity*. This principle, advanced by Froebel, is now so thoroughly established in education that it may almost be taken as self-evident. Self-activity is the keynote of modern pedagogy. And yet, while recognized in theory, this fundamental principle is often almost totally neglected in practise. In the light of this principle of education by self-activity let us review briefly certain phases of methods in medical teaching.

In developing the self-activity of the student, it is evident that the methods first of all must arouse his interest and attention. Interest we know to be most intense in things which satisfy conscious needs. Now the medical student wants above all to be a good practitioner. If he knows that a certain thing will help him to accomplish this, he is intensely interested, and will exert an active effort to secure it. "The mind interprets impressions from without, not according to their intrinsic nature, but accordingly to their relation to the needs of the organism" (Bagley). Common sense and good pedagogy therefore agree that in teaching any subject in the medical curriculum, the teacher should make sure that the student realizes its bearing upon his later work.

Some may conclude from the foregoing that, since time is limited, only the so-called

[2] In addition to the education of practitioners, the medical school has other important functions, such as the advancement of medical science through original investigation, but these are not within the scope of the present paper.

"practical" facts, those that are of obvious utility in the practise of medicine, should be taught, and that no time should be wasted on "theoretical" aspects. While this argument may appear plausible at first glance, its fallacy is apparent on closer examination. In the first place, it is impossible in any given subject to select out only those facts which may later be needed. Moreover, even if such facts could be selected, it would be impossible to teach them as bare, empirical facts, in such a way that the student could understand, remember and utilize them, without a comprehension of the science of which they form a part. The "theoretical," as Bagley states,

> contributes to the coherence of the various facts and principles as *knowledge*. Its value can not be disputed, for any attempt to "cut out" the "impractical" parts invariably results in the inefficient functioning of the remainder. Short courses that give only the essentials, fifth-rate colleges and normal schools that educate you while you wait, are sufficiently damned by their own products.

There is, it must be acknowledged, some truth in both the "practical" and the "theoretical" points of view. The best methods of teaching will, therefore, utilize both. While each subject should be taught from the theoretical, scientific point of view, at the same time its practical application should be kept constantly in mind. In selecting material to develop the essential principles, those facts should be chosen which will also probably be of greatest intrinsic value for later work. Anatomy, for example, should be taught, not as a mass of empirical facts, but as a special branch of biological science. But in selecting from the huge mass of available data the facts necessary to illustrate the science of anatomy, so far as possible those facts should be chosen that are also of direct, intrinsic value in physiology, pathology and clinical medicine.

If this plan were consistently followed out, and everything excluded excepting facts, especially those of intrinsic value, necessary to develop a scientific basis, a "working-knowledge," for each branch of study, the amount of subject-matter presented in each could be greatly reduced. We all recognize that the curriculum is now overloaded. It is impossible to teach so much and teach it well. "What men need" (according to Huxley) "is as much knowledge as they can assimilate and organize into a train for action."

To develop in accordance with the foregoing plan the most effective methods of teaching, it is evident that each teacher must understand the curriculum as a whole. The laboratory man must be familiar with the clinical work. But this is not all. Since good teaching must take into account that which has gone before as well as that which is to follow, it is equally evident that the clinical man must be familiar with laboratory subjects and methods. We can not expect the best results in medical education until there is a better understanding and more cooperation between teachers of the various subjects all along the line. As medicine progresses, all phases appear more clearly as varied manifestations of the same underlying biological science, and only when this is realized will the clinical and laboratory work be more closely knitted together.

We have seen that to interest the student and arouse him to self-activity, he should be made to realize that each subject contributes an essential part in training him for the desired end. We may next inquire as to *how* he must be self-active. Since efficiency in practise consists in accurate observation and reasoning, resulting in wise action in dealing with medical problems, his training should develop self-activity in these very lines. He must observe, think and act for himself. For this purpose almost ideal facilities exist in our laboratories and clinics. Unfortunately, however, we are far from utilizing these facilities to their fullest extent. Our methods fail to make the student self-active, especially in observation and reasoning.

First we may consider observation. This can be cultivated only by actual observation of medical phenomena on the part of the student. It is, however, a surprising fact that in many laboratories and clinics there is no opportunity for the student to make an *orig-*

inal observation. Why? Simply because through a pernicious lecture system he has already been told all about what he is to see, before he has ever had a chance to observe it for himself.

It is furthermore a fundamental law of learning (technically the doctrine of apperception) that we can not comprehend new facts except upon the basis and in terms of previous concrete experience. Hence the dictum: "In teaching, always proceed from the concrete to the abstract; from the particulars to the general; from the known to the unknown." It is therefore evident that to give lectures preceding practical objective study not only prevents the exercise of original observation but also inverts the normal procedure in the process of learning.

Much time and energy is sometimes thus wasted in trying to teach by lectures what would be quickly and easily comprehended *after* the fundamental data had been acquired by objective study. In some schools, for example, the junior year is largely given over to lectures and other didactic work which is supposed to prepare the students for the actual clinical work, the latter being chiefly concentrated in the senior year. This, it seems to me, is a fundamental mistake. If there were only one alternative, it would be better to reverse this order, giving the clinics first, and the lectures later. In actual practise, however, they are best intermingled and closely correlated, care being taken always to provide the objective basis before the more abstract generalizations are considered.

Even when the practical work is placed first, however, it by no means follows that adequate training in observation will result. In both laboratories and clinics it is a common practise as a preliminary step to *tell* the student (either orally or by printed guides) what he is to see. The student thus is not required, and indeed has no opportunity, to observe for himself. Practically all there is left for him to do is to verify what he has already been told. However valuable this may be, it does *not* develop power of original observation. It is, of course, desirable to precede all practical work with a brief introduction which will enable the student to proceed intelligently with his work. Such an introduction, however, should be merely for the purpose of explaining technical procedure and of raising questions the answer to which the student should seek by original observation.

The ideal plan is thus for the student to work out everything for himself by the method of discovery. This applies not only to the original observations, but also to the later process of reasoning, whereby we proceed from particular data to general conclusions, and thence to rational action. The method of self-activity may therefore be expressed in a negative way by the following practical rules: Never tell a student anything he can observe for himself; never draw a conclusion or solve a problem which he can be led to reason out for himself; and never do anything for him that he can do for himself.

Unfortunately, however, there are limitations to the application of this method. It is difficult to apply successfully, requiring skill and experienced judgment on the part of the teacher. Lack of time would moreover prevent the student from repeating the history of the race by the method of discovery. But though difficult and slow at first, by working out for himself at least the fundamental data, a solid basis is laid which makes possible more rapid progress later. Time lost at the beginning is thus time gained in the end. We should therefore insist that *so far as practicable* this ideal method be applied for the purpose of training the student to self-activity, in developing his ability in observation, reasoning and action.

As supplementary to the foregoing, it is usually necessary to adopt easier though less effective methods of instruction. Thus where necessary data can not be secured by original observation, they may be supplied by the usual type of laboratory or clinical demonstrations, which the student can verify. Next in value below this as a means of imparting knowledge comes the informal lecture or recitation, illustrated by demonstrations, models, pictures, etc. Next comes the

text-book, and lowest of all in the scale is the formal lecture. Curiously enough, the lecture is also the *easiest* method, by which *apparently* the greatest amount of information is gained with the least expenditure of energy, at least on the part of the student. But this is a delusion. The knowledge thus gained is unreal and transient. It is "in at one ear, and out at the other." As we should naturally expect from the principle of self-activity, the ease of the method is apt to be inversely proportional to the efficiency of the instruction. In order therefore to train our students most efficiently in self-activity, we should use the maximum amount of the more difficult but more effective methods and the minimum of those easier but relatively inefficient.

This will perhaps be made clearer by a brief illustration from personal experience. To learn, for example, the normal histology of any given organ by means of a stained and mounted section, this should first be studied by *original observation*. The students observe the structure with naked eye, low power and high power of the microscope, and without previous description by teacher, book or laboratory outline. They record their observations by sketches and brief notes. The aid of the teacher at this stage should be restricted to questioning the individual students so as to recall related facts previously studied and prevent the student from going too far astray. At first, students are apt to be utterly helpless when thus thrown largely upon their own resources, but they soon develop surprising powers of observation. This "investigation" occupies the first part of the laboratory period. The teacher then informs the class regarding the section they have studied, and discusses briefly their mistakes of observation. He directs them in restudying the section, and in correcting their mistakes. The students now extend their knowledge by verifying the statements found in their text-books. Demonstrations are made to furnish additional data and elucidate the more difficult points. Drawings are finally made by the students, to fix the corrected impressions upon their minds. At a later class-conference, the students are led to review the facts learned, to correlate and interpret them and to reason out general conclusions or laws of structure. These laws they utilize and apply in the subsequent work. Brief written reviews are also frequently held. Occasional lectures by the teacher elucidate the more difficult phases, and indicate the relations of histology to physiology, pathology and clinical medicine. The results are satisfactory as shown by final examination, both written and practical, and by the extent to which the students are able to retain and utilize their knowledge in later work.

The foregoing method illustrates how students may be trained to self-activity in observation and reasoning, and to a certain extent in application. The application of the generalizations reached by observation plus reasoning, while essential in every subject, is especially characteristic of the clinical work. That the student should be self-active in his clinical work, that to acquire skill in the practical application of his previous knowledge he must "learn by doing," is universally recognized. It is therefore unnecessary to dwell upon this phase of the subject. It may be worth while, however, to remember that, above all, in the clinics, "the main business of the teacher is to render his services unnecessary" (Strayer).

To summarize the foregoing: it has been maintained that in medical education there is great need of more effective methods of teaching. Efficient teaching requires a clear view of the ultimate aim, which in medicine is to train efficient practitioners. To accomplish this aim, rational methods of teaching should develop in the student self-activity in observation, reasoning and action. While some may be unable to accept fully the ideas here presented, all will surely agree that great improvement would result if medical teachers would study more carefully their educational methods. The younger teachers who are so fortunately located could greatly improve their efficiency by taking work in the schools of education connected with the various uni-

versities. Those unable to do this should at least study the principles of pedagogy, which are available in numerous books. Although pedagogical literature deals chiefly with elementary, rather than advanced or professional education, it is nevertheless of great service, for the same fundamental principles extend throughout, from the kindergarten to the university. Among those books which may be recommended as helpful are the following: Spencer, "Essays on Education" (a recent edition, with introduction by ex-President Eliot, in the "Everyman's Library" series); James, "Talks to Teachers on Psychology," etc. (Holt); Charters, "Methods of Teaching Developed from a Functional Standpoint" (Row, Peterson & Co.); Bagley, "The Educative Process" (Macmillan); Thorndike, "The Principles of Teaching Based on Psychology" (Seiler). And in conclusion, permit me to suggest that a more thorough discussion of educational methods and principles in our association meetings, and also in the faculty meetings of our various medical schools, would result in greater efficiency in our teaching.

C. M. JACKSON
UNIVERSITY OF MISSOURI

SCIENTIFIC NOTES AND NEWS

PROFESSOR ABBOTT LAWRENCE ROTCH, founder and director of the Blue Hill Meteorological Observatory and professor of meteorology at Harvard University, died on April 7, aged fifty-one years.

DR. JOHN H. MUSSER, professor of clinical medicine in the University of Pennsylvania and one of the most eminent physicians of Philadelphia, died on April 3, in his fifty-seventh year.

THE second annual award of the Willard-Gibbs Medal, founded by Mr. William A. Converse, will be made by the Chicago Section of the American Chemical Society on May 17, to Professor Theodore W. Richards, of Harvard University. It may be remembered that the initial award of this medal was made last May to Professor Svante Arrhenius. Professor Richards has chosen for the subject of his address "Atomic Weights." An invitation is extended to all members of the American Chemical Society, who desire to be present on the occasion of this award.

SIR J. J. THOMSON has been elected a foreign member of the Naples Academy of Sciences.

THE clay model for a bust of Commander R. E. Peary has been executed by Mr. William Couper and is now on its way to Florence to be cut in Carrara marble. The bust is a gift to the American Museum of Natural History from Mrs. Morris K. Jesup and will take its place among the other marble busts in the niches in memorial hall.

THE Paris Geographical Society will present Dr. Charcot with its gold medal for the work achieved in South Polar exploration by the *Pourquoi Pas* expedition.

THE council of the New Zealand Institute, at its annual meeting held in Christchurch at the end of January, decided to award the Sir James Hector memorial medal and prize to Dr. L. Cockayne as the investigator, working in New Zealand, who has done most to advance botanical science.

THE Academy of Science, the medical faculty of the University of Havana and several other scientific societies and institutions have passed a joint resolution in which the names of Dr. Carlos J. Finlay and Dr. Aristides Agramonte are presented to the Nobel Prize Commission as candidates for the prize to be awarded in 1912. The resolution points out that Dr. Finlay was the first to claim that yellow fever is transmitted by the mosquito, while Dr. Agramonte is the sole survivor of the United States Army Board composed of Drs. Reed, Carroll, Lazear and himself, which demonstrated the correctness of this theory.

THE magazine *Good Housekeeping* announces that Dr. Harvey W. Wiley, formerly chief chemist of the United States Department of Agriculture, became contributing editor of that magazine on April 1. The magazine has established at Washington a "Bu-

reau of Foods, Sanitation and Health," of which Dr. Wiley will be director.

Instructors and students of the forestry department of the University of Michigan entertained Professor Filibert Roth recently at a farewell dinner. Professor Roth's resignation from the University of Michigan will take effect in June, and he will become the head of the forestry department of the College of Agriculture at Cornell University next fall.

Dr. J. C. Willis has retired from the directorship of the Royal Botanic Gardens, Peradeniya, Ceylon, to become director of the Botanic Gardens at Rio de Janeiro.

Dr. Eugen Kuehnemann, professor of philosophy at the University of Breslau, Germany, and recently German exchange professor at Harvard University, has been appointed as the first German university professor to occupy the Carl Schurz memorial professorship established last year in the University of Wisconsin by German-American citizens of Wisconsin and friends of the university.

Dr. Severance Burrage has resigned as professor of sanitary science in Purdue University, Lafayette, and will spend several months in Europe, after which he will take charge of the biologic and serum department for an Indianapolis drug house.

Professor F. W. Carpenter, of the University of Illinois, department of zoology, has been appointed American reviewer for *Zentralblatt für Normale Anatomie und Mikrotechnic.*

Professor John T. Stewart, of the University of Minnesota, has been authorized by the regents to attend the National Drainage Congress in New Orleans this month at an expense not to exceeed $100.

Mr. Donald F. MacDonald, geologist to the Isthmian Canal Commission, has been granted three months leave of absence from his duties with the Commission to make a geological study of the disputed territory between the republics of Panama and Costa Rica. Mr. MacDonald sailed from Colon for Bocas del Toro, near the Costa Rician border, on April 1. From there he will proceed by small boat and by native porters to the region to be examined.

According to a cablegram to the New York *Times* the Austrian Red Cross Society will be represented at the International Conference of Red Cross Societies, which meets in Washington on May 7 to 17, by Dr. Maurice Victor Silbermark and Dr. Yetta Silbermark Reissing, who sail by the *Auguste Victoria* on April 16.

Mr. Arthur S. Watts was elected president and Edward Orton, Jr., secretary of the American Ceramic Society at the recent Chicago meeting.

Professor George Grant MacCurdy will be the delegate from Yale University to the International Congress of Anthropology and Prehistoric Archeology to be held in Geneva, Switzerland, during the first week of September, 1912.

The following delegates have been appointed to represent the American Philosophical Society on the following occasions: Vice-president William B. Scott, of Princeton, to represent the society at the two hundred and fiftieth anniversary of the foundation of the Royal Society in July next; Professors Paul Haupt, of Baltimore, E. Washburn Hopkins, of New Haven, Morris Jastrow Jr., of Philadelphia, and A. V. Williams Jackson, of New York, as delegates to the eleventh International Congress of Orientalists, to be held at Athens on April 7 to 14; Dr. Franz Boas, of New York, a delegate to the eighteenth International Congress of Americanists, to be held in London from May 27 to June 1. At the centenary of the Academy of Natural Sciences on March 19 to 21 the society was officially represented by Professor Henry F. Osborn, of New York, Dr. Charles D. Walcott, of Washington, Mr. Samuel Vauclain, of Philadelphia, Professor Wm. Bullock Clark, of Baltimore, and Dr. Henry H. Donaldson, of Philadelphia.

THE tenth annual meeting of the South African Association for the Advancement of Science will be held in Port Elizabeth from July 1 to 6, under the presidency of Dr. A. Theiler. The sections and their presidents are as follows: A, astronomy, mathematics, physics, meteorology, geodesy, surveying, engineering, architecture and irrigation, Mr. H. J. Holder; B, chemistry, geology, metallurgy, mineralogy and geography, Professor B. de St. J. van der Riet; C, bacteriology, botany, zoology, agriculture, forestry, physiology, hygiene and sanitary science, Mr. F. W. FitzSimons; D, anthropology, ethnology, education, history, mental science, philology, political economy, sociology and statistics, Mr. W. A. Way.

THE fifth of the Weir Mitchell lectures of the College of Physicians, Philadelphia, was delivered on March 29 in Mitchell Hall, by Dr. William H. Howell, of the Johns Hopkins University, on "The Factors concerned in the Coagulation of Blood and their Variations under Pathologic Conditions."

UNIVERSITY AND EDUCATIONAL NEWS

THE board of directors of the Knights of Columbus announces that $385,000 of a $500,000 endowment fund for the Catholic University in Washington already is in hand. The income from this fund, the remaining $115,000 of which has been subscribed, will be used to provide scholarships for fifty students.

THE Berlin correspondent of the *Journal* of the American Medical Association writes that according to the official estimate of the expenses of the Prussian universities, for the fiscal year 1912, they will amount to $5,016,389 (20,065,556 Marks). Of this amount $2,202,058 is to be spent for institutes, collections and the university religious services. For salaries, remuneration and other personal expenses of the professors, instructors, officers, assistants and employees $2,059,466 are allowed and for lodging allowance $288,687. For the expenses of the academic management $93,266 are appropriated, for the building expenses $200,275 and for taxes $9,868. For contingent expenses $110,970 are provided, and $51,195 are appropriated for relief and stipends. The entire expenses are distributed among the individual universities as follows: Berlin, $1,184,515; Breslau, $541,311; Halle, $546,982; Bonn $485,146; Kiel, $482,741; Göttingen, $463,215; Königsberg, $411,621; Greifswald, $366,475; Marburg, $348,326; Münster, $168,158, and Braunsberg, $17,395. The expenses are to be met by the following income: from the state fund, $3,636,801; from endowments and other funds, $178,928; interest from capital and the income from real estate, $119,619, and from their own earnings, $1,081,039.

PLANS are well under way for the new building for the department of clinical medicine at the University of Wisconsin. The department exists for the purpose of looking after the health of the students in the university. There is a corps of five doctors and four trained nurses who are kept busy ministering to sick students or taking precautionary measures in the case of those exposed to disease. The new building will have ten offices on the first floor for the treatment of common ailments, and in the basement will be a sterilization room and special treatment rooms, fitted up with X-ray machines, baking machines and other special equipment. The value of having a department to look after the health of students is shown by the fact that there have been no epidemic diseases among the students that were not controlled as soon as the first cases appeared, since the establishment of the department. Previous to its establishment, there were a number of bad epidemics among students, the most serious of which was an outbreak of typhoid fever in 1907 which resulted in the death of several students.

THE *Journal* of the American Medical Association states that the professors of the University of Dijon have unanimously passed a resolution calling for a university congress to organize the French universities more closely and to extend their influence abroad. There has been founded under the auspices of the France-Amérique committee a league to foster French interests in America, which

has for one of its objects to make known to the public of the two Americas the value of French general culture and practical instruction.

Dr. Charles E. Marshall, professor of bacteriology and hygiene in the Michigan Agricultural College, has accepted an appointment as director of the graduate school and professor of microbiology, at the Massachusetts Agricultural College. He will begin his new duties on September 1. The Graduate School of the Massachusetts Agricultural College is an outgrowth of graduate work started under the direction of Professor Charles H. Fernald nearly eleven years ago. Four years ago the school was organized with Professor Fernald as director, and since his retirement Dr. Henry T. Fernald has been acting director. During the existence of the school fifteen men have been given the degree of master of science, and seven, the degree of doctor of philosophy. At the present time sixteen men are enrolled as graduate students. It is the intention of the trustees to develop the activities of the school.

Dr. Raymond A. Pearson, recently Commissioner of Agriculture for the state of New York, has accepted the presidency of the Iowa State College of Agriculture at Ames. Dr. Pearson has been granted leave of absence for the summer and will visit agricultural colleges in Europe.

Dr. Thomas McCrae, A.B., M.D., Toronto, associate professor of medicine at the Johns Hopkins University, has been appointed professor of medicine in Jefferson Medical College, Philadelphia, to fill the chair vacant by the resignation of Professor James C. Wilson.

At the University of London Professor F. G. Donnan, F.R.S., has been appointed to the chair of general chemistry at University College, in succession to Sir William Ramsay. Dr. L. N. G. Filon, F.R.S., has been appointed to the Goldsmid chair of applied mathematics and mechanics to succeed Professor Karl Pearson, who resigned this chair on his appointment to the Galton chair of eugenics.

DISCUSSION AND CORRESPONDENCE

ARE HORNS IN SHEEP A SEX-LIMITED CHARACTER?

Arkell and Davenport in Science for March 8, 1912, answer this question in the affirmative on the basis of certain crosses which they have made between horned and hornless races. In doing so they call in question the authenticity of a statement made by me in a recent publication as follows:

> In merino sheep the male has well-developed horns, but the female is hornless; yet if the male is castrated early in life no horns are formed.

They comment thus:

> He gives no reference for the last statement; and in view of the variability of the horned condition in the males of the "merinos" the conditions of the experiments would have to be carefully considered before such a result could be accepted as settling the question of the dependence of horns in heterozygous males upon a secretion from the testis.

In reply to this criticism, I beg to say that I gave no authority for the statement in question because I can myself vouch for it. I grew up on a farm where Merino sheep were kept in considerable numbers. From my earliest recollection until I was 21 years old I saw the operation of castration practised each year on 50 or more ram lambs and its effects were perfectly familiar to me. The result is exactly that stated. If the male is castrated early in life, say within a month after birth, no horns develop. If castration has been delayed for two or three months, the horns begin to grow, but castration then promptly arrests their growth. I can recall but one exceptional case in the hundreds that came under my observation. In that case the horns continued to grow for some weeks. When this case was observed, the animal was caught and found to have been imperfectly castrated. A second operation caused cessation of the horn development. My father used registered Atwood Merino rams, and his ewes were pure bred. The ewes were regularly hornless as they are typically in this breed. See figures in Robert Wallace's (1907) "Farm Live Stock of Great Britain," p. 592. The males were as regularly horned, if not castrated. We usually

raised several of these (uncastrated) males each year and they were remarkably uniform in horn development. Compare again Wallace's figures, which are accurate, being photographs. Arkell and Davenport's statement concerning the "variability of the horned condition in the males of the Merinos" leads me to think they must refer to *grade* Merinos, certainly not to the pure bred ones.

To my mind the evidence is clear that in pure Merino sheep castration does prevent development of the horns, and I have no doubt that in other breeds also castration has similar though perhaps less conspicuous effects. In breeds which are horned in both sexes the males regularly have *better developed* horns than the females, and castrated males have smaller horns than uncastrated ones. See figures in Wallace.

If castration has the effect stated, the assumed nuclear *inhibiting* factor of Arkell and Davenport is quite superfluous. Their experimental results are fully in accord with those of Wood and are fully covered by the simple statement of Bateson that the horned character is "*dominant in males and recessive in females.*" Why this is so I have attempted to point out. Presence of the testicle is necessary for full horn development, in some breeds it is necessary for *any* horn development. Reasoning from the experimental results obtained in poultry it seems probable that injections of testicle extract into the female would cause increased horn development similar to that of the male. It would be interesting to know whether the testicle of all breeds would behave alike in this experiment. Whether the female sex gland acts as an inhibitor of horn development would be a wholly different question, yet one capable also of experimental solution.

To assume, as Arkell and Davenport do, that inhibiting factors present in *X*-chromosomes affect the horn development seems to me unwarranted, for the simple reason that neither inhibitors nor *X*-chromosomes are known to exist in sheep. That Guyer has recognized the existence of an *X*-chromosome in man has no very direct bearing on the question, but even Guyer's result is unconfirmed by Gutherz, who has reinvestigated the spermatogenesis of man upon exceptionally favorable material.

Arkell and Davenport reason thus:

> The results of the table [of crosses] accord very closely with expectation, so that we are justified in concluding that an explanation of the results like that we offer is the correct one.

But Bateson's explanation accords also; wherein lies the superiority of the new one offered? To establish the probable correctness of a hypothesis it must be shown that no other hypothesis accords with observed facts equally well. Has this been shown in the case before us?

Consider how one unproved hypothesis has been added to another. First it is assumed that in hornless animals a gene for horns has either been lost or is inhibited. It is equally probable that no gene has been lost and that nothing is inhibited. Secondly, it is assumed that one inhibitor is inferior to one horn-gene in power, but that two inhibitors surpass one horn-gene, yet two inhibitors are themselves overpowered by two horn-genes; without all three of these ungrounded assumptions of the relative valency of imaginary genes the explanation fails altogether. Further, it is assumed that the female is capable of carrying two inhibitors, but the male only one. And finally when this colossal structure of hypothesis encounters one well-known physiological fact, the result of castration, that fact is calmly brushed aside. Is this a desirable extension of Mendelian interpretation?

W. E. CASTLE

March 13, 1912

THE MOTH OF THE COTTON WORM (ALABAMA ARGILLACEA HUBN.)

To THE EDITOR OF SCIENCE: In connection with the notices appearing in SCIENCE (October 16, November 10 and December 29), concerning the moth of the cotton worm and the destructive work of the caterpillar on cotton, a note from Missouri may be of interest.

During the fall this moth was present in great numbers in various parts of the state.

The northward migration seems to have been quite general. They made their first appearance in the vicinity of Kansas City early in September, and by September 26–30 were present in countless swarms at Kansas City and Holden. They reached Macon in north Missouri September 24, and were very abundant and troublesome until frost. Here at Columbia they were especially abundant the last two weeks of September and early October; while later in October the moth of the army worm (*Leucania unipunctata*) was far more abundant, collecting about cider mills and injured and decaying fruit. With conditions favorable next year, we may expect considerable injury from the army worm in this state.

One point with reference to the moth of the cotton worm which the other notes have not brought out, is the injury which they do to ripening fruit in the orchard and where fruit is exposed in the market. This has been especially emphasized in all the letters received at this office this fall. As is well known, this moth has rudimentary mandibles by means of which it can break the skin of fruit and then with its proboscis it sucks out the juices. Late peaches, especially Heath Clings, are reported as having been severely injured this year. In some cases a dozen or more at a time collect on a single peach and eventually all the juice is consumed, leaving only the skin, pulp and pit. Grapes and even bananas in the market are attacked. In the orchard, after the peaches were picked, the moths turned to the apples. Their attack on the apple is similar to that on the peach except that the juice is drawn out in patches which turn brown and become mellow like bruises. The affected patches vary from the size of a pea to that of a dime or a quarter. The most of the fruit so attacked decays.

The strange northward migration of this moth which has always been of considerable interest to the entomologist has proved to be of special interest to many of the Missouri fruit growers this year.

Leonard Haseman
Department of Entomology,
University of Missouri

CRYSTALLOGRAPHIC TABLES

To the Editor of Science: As a teacher of crystallography I have found that students rarely appreciate the full significance of the fundamental laws of the science until, by actual measurement and calculation, they have found concrete evidence. With large crystals of quartz, calcite, tourmaline, zircon, rutile, barite, staurolite and a number of others, satisfactory results may be obtained by use of the Penfield hand goniometer. The advantage of such crystal measurement is twofold; it illustrates the laws which govern the arrangement of crystal planes, and it teaches the value of the science as a means of mineral determination.

Regarding the latter phase of the study, students are taught the methods of measurement and calculation necessary in each system for the determination of axial ratios. To bring out clearly the real value of such calculations as a means of practical mineral determination some sort of reference table of axial ratios seems desirable. Such tables have been compiled in a somewhat imperfect form, and it is to these that attention is directed.

The axial ratios of common tetragonal and hexagonal minerals are arranged in ascending values of c, the mineral names being placed in a parallel column. In practise the chart is placed before a class with the mineral names covered. After careful measurement and calculation the student refers to the column of ratios, and the fact that he can, in many cases, determine the mineral properly by this means alone, makes it a most illuminating and interesting exercise.

The orthorhombic system presents considerable difficulty in compiling a table of ratios since there are three possible ways in which the values of a, b and c may be arranged and still be in accord with the convention that b must be greater than a. Having determined the axial ratios, one is in doubt as to the proper arrangement. The three possible values of a, which may occur, b being unity, are the value of a when (1) c is greater than a and also greater than b; (2) c is greater than a and less than b; (3) c is less than a

and less than *b*. The most useful table, one which precludes the necessity of rearrangement or recalculation, is one which includes all three of these possible values. For the most common orthorhombic minerals such a table has been arranged with the values of *a* in increasing order of magnitude, the corresponding values of *c* and the mineral names being placed in parallel columns. Thus each mineral appears three times, and the value of *a* accepted by convention is underlined. The corresponding value of *c* in the parallel column is a very useful check in tracing the unknown mineral.

If a monoclinic mineral is held with the greatest possible number of faces vertical, the most probable mistake in orientation is the interchange of *a* and *c* axes. Hence the table is made to include both *a* and *c* as possible values of *a*, *i. e.*, each mineral appearing twice in the table.

Would such tables, enlarged to include all minerals for which axial ratios have been determined, be useful accessories in the work of crystallographic mineral determination with the reflecting goniometer? Would tables further enlarged to include artificial crystals be of use to the chemical crystallographer?

Before undertaking such a task one wishes to know if it is worth while, and for this reason the questions are presented. Suggestions, and the utmost freedom of criticism by teachers of crystallography, are invited.

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HOUSE AIR

TO THE EDITOR OF SCIENCE: I had occasion recently to consult the issue of SCIENCE for September 29, 1911, and read for the first time the letter on "House Air" from Professor J. Y. Bergen, of Cambridge.

He makes this statement regarding the ventilation obtained from a hot air register:

> The ventilation . . . is much better than can be obtained in summer by opening a single window to its full height.

It is doubtless known to him and should be more widely known that it is better to pull the window down a couple of inches from the top and up from the bottom when ventilation is required than to open either half only. The truth of this statement can easily be tested by holding a lighted candle at the window openings.

G. L. MANNING
ROBERT COLLEGE,
January 23, 1912

QUOTATIONS

MOST RECENT INVESTIGATIONS ON THE DETERMINATION, PRESERVATIVE ACTION AND ADMISSIBILITY OF THE USE OF BENZOIC ACID[1]

PART II[2]

I NOW come to the most important part of my work—a critical summary of the three detailed investigations on the effect of benzoic acid and of benzoates on man which have been carried out during the last four years and which now furnish us that broad basis, which I have always desired, necessary for the formation of an intelligent opinion. First, there are two great works from the American Department of Agriculture; one carried out by H. W. Wiley,[3] in 1908, the other by a commission, under the chairmanship of the distinguished chemist, Ira Remsen, consisting of the three well-known American scientists, Professor Russell H. Chittenden, of Yale University; Professor John H. Long, of Northwestern University, and Professor Christian A. Herter, of Columbia University, New York. It seems strange that a great government should publish two books, one right after the other, dealing with the same subject-matter; and we seek in vain, in the second large volume of 761 pages, for a word of explanation of this surprising fact. Wiley's work is simply ig-

[1] Translated from the *Chemiker-Zeitung*, Cöthen, November 28, 1911, pp. 1314–17.

[2] Part I. (*Chem. Ztg.*, 1911, pp. 1297–99) is a summary of the articles dealing with the isolation, qualitative and quantitative determination, natural occurrence in plants, preservative action, use and toxic effects of benzoic acid.

[3] *J. Soc. Chem. Ind.*, 28, 67 (1909).

nored in the publication of Chittenden, Long and Herter, as if it had never existed! From this it may be concluded at once that the results or conclusions of Wiley did not seem, to the second commission, worth discussing. This is so much the more surprising in that Wiley, under the authority of the Department of Agriculture, has, for years, been publishing similar investigations on boric acid, salicylic acid, etc. Under these circumstances I had at first thought of leaving Wiley's work entirely out of consideration, but since Dr. V. Gerlach, of Wiesbaden, in the third[4] of the detailed publications mentioned above, expresses his regret that he had access to only a short abstract of Wiley's investigation, a discussion of the original, which now lies before me, will be appropriate, especially as the work will become even more difficultly accessible in the future.

Wiley studied the effect of benzoic acid on six persons and that of sodium benzoate on six others. In each case a 10-day period without the preservative was followed by a 5-day period with 1 gram, then by periods of equal length with 1.5, 2.0 and 2.5 grams, each, of the preservative, and finally by another 10-day period without the preservative; *i. e.*, 40 days, in 20 of which benzoic acid was administered. Above all, it is to be regretted that the preparations were given in capsules and that the small quantities (0.25–0.5 gram) which, in practise, are most important were not administered, as the minimal dose was 1 gram; and finally, that the experiments were extended only over a relatively short time. As far as concerns the results, I cling to the good custom of not doubting an investigator's facts and analytical data; only the basis and logic of the conclusions are here discussed. From the experiments, which are reported carefully and in considerable detail in 250 pages, Wiley concludes some very disadvantageous things for benzoic acid. He finds:

> It is evident that the use of benzoic acid, either as such or in the form of sodium benzoate, is highly objectionable and that it produces grave disturbances of the metabolism and health (irritation of the stomach, nausea, headache and, in a few cases, vomiting). So much the more importance is placed on these symptoms as they were observed in healthy men, well and carefully fed. All the test persons showed a distinct loss in weight and all the more or less marked changes in metabolism shown by investigation of the urine and feces were always of such a nature that they could never be regarded as a favorable change.

In my opinion, this sentence might just as well read: Careful investigation of the urine, feces and metabolism did not show a single symptom from which a disturbance due to the partaking of benzoic acid might be deduced! That the normal condition could not be improved by benzoic acid, as Wiley seems to require, is not surprising. As Wiley is thus far the only investigator who, supported by comprehensive experiments, has formed a condemnatory opinion of benzoic acid on toxicological grounds, I have taken the trouble to criticize his work somewhat more closely and to examine the value of his arguments. The very thorough examination of the urine, which is reported in many tables, showed nothing special. Who could lay much value on the fact that in the benzoic-acid periods the amount of solid substances in the urine increases 2.3 grams, *i. e.*, from 55.5 to 57.8, when, in the first place, the diet is not strictly fixed and, secondly, the addition of 1–2 grams of benzoic acid per day almost completely covers the increase. The relation of sulphur to nitrogen, sulphuric acid to nitrogen and phosphoric acid to nitrogen remained entirely unchanged; with the arbitrary diet the variations of 1–3 per cent., either way, of the values are unavoidable. The alkylsulphuric acids remained perfectly constant, the absolute amount of urinary sulphur changed only to an insignificant degree explainable by the method of procedure; a decrease of neutral sulphur from 14 to 12.4 per cent. in the benzoic acid period and to 11.1 per cent. in the after period seems to be proved, while on the other hand the total sulphur shows an increase from 85.9 to 87.7 and 88.9 per cent. But any one wanting to

[4] "Physiologische Wirkungen der Benzoesäure und des benzoesauren Natrons," Verlag von H. Staadt, Wiesbaden, 1909.

draw any conclusions from this might just as well, instead of supposing, like Wiley, an increase of oxidation, infer that the oxidizing power of the body is distinctly increased a little by rather large doses of benzoic acid and increases still more after the excretion of the benzoic acid, a sign of an especially powerful cell activity. It is an interesting fact that the separation of benzoic acid as hippuric acid does not take place very quickly but that an after period following a rather prolonged benzoic acid diet still shows increased hippuric acid. The microscopic investigation of the urine—the method is not described in detail—was made six times in all for each of the twelve persons studied—once each in the fore and after periods and four times in the benzoic acid periods. The results are represented by numbers: 0 = nothing, 1 = very little, 2 = little, 3 = considerable, 4 = much, 5 = very much. The following substances were sought for: crystals of uric acid, oxalates and phosphates, amorphous phosphates, epithelial cells and leucocytes, hyaline cylinders, finely granulated and coarsely granulated cylinders, mucous cylinders, mucous fibers. The numbers were added for the twelve persons and divided by twelve, and the values so obtained compared in percentages! According to the mean values most of the organized elements are increased somewhat; leaving out the crystals, the leucocytes and the finely and coarsely granulated cylinders are somewhat increased, the hyaline cylinders somewhat diminished and the mucous fibers hardly appreciably increased. Considering how strongly a 2 influences the 1's and 0's which are found in greater number, it is preferable to consider more closely each individual constituent of the six series. When this is done it is found that, excepting the mucous cylinders (increase of 50 per cent.), nothing at all remains of all the construed increases. But as the mucous fibers do not markedly increase, I set no importance on the increase of the mucous cylinders. Wiley, however, concludes that there is a slight tendency towards an increase of renal activity during the benzoic acid periods. This "slight tendency" to cause everything that is possibly bad is construed everywhere that Wiley can say nothing definite. In the feces nothing remarkable was found. On an average, in the experiments with benzoic acid the feces were a trifle drier, in the benzoate experiments somewhat damper, during and after the administration of the preservative than before it was given.

That the majority of Wiley's twelve persons should have lost 0.5–2.0 kilograms on a freely chosen diet is very remarkable, and we should much like to learn more as to the principles on which the amount of food was measured. The amounts of dry substance, fat, nitrogen and calories daily taken in the individual periods vary not inconsiderably—with a "faint tendency" in most cases to decrease in the course of the forty days—which alone explains the slight decreases in weight. We do not learn why a greater regularity in the taking of food was not striven for. I reproduce some particulars concerning one person: Test Person 1 (C. W. N.) had nothing particular to complain of against benzoic acid, but found that his strength decreased markedly, so that he could hardly do his work. At the same time, however, it is stated that this person was often very hungry, that the abundant and varied food with 4,000 calories was not sufficient for him. On the other hand, it is seen from page 1090 that during the fore period this person partook of 606 grams of dry food, on the average during the total benzoic acid periods, of 590 grams, and in the after period, of only 567 grams. Why more food was not given him remains incomprehensible, as he had already decreased 0.5 kilogram in weight in the second 5-day fore period and 0.3 kilogram in the first 5-day benzoic acid period and was hungry! In this record I see nothing but decreased body weight and consequent feeling of weakness, probably increased by suggestion, resulting from a somewhat too limited consumption of food. To other persons was given 1 per cent. of the body weight in dry substance, to No. 1 only 0.87 per cent. On page 1166 we learn, to our surprise, that Person 1, who daily took about 13–14 grams of nitrogen, daily gained

(in spite of decrease in weight) 0.53 gram nitrogen in the fore period, 0.39 gram in the benzoic acid period and 1.41 grams in the after period, and therefore apparently became poorer in fat and richer in albumin. And we learn further that Person 1 did not at all stand alone in this respect. In spite of decreases in weight and disturbances of metabolism of subjective nature, five of the twelve persons increased, seven decreased, in nitrogen; in the average of all twelve experiments there was even, in all the periods and sub-periods of the 40-day test, a daily increase of 1.2–1.5 grams of nitrogen. The nitrogen was used to the best advantage; on an average for all the test persons, 7.26 per cent. was excreted in the feces in the fore period, 7.44 in the benzoic acid period and 8.16 in the after period. The phosphoric acid metabolism was hardly influenced, but it is true, as Wiley brings out, that during the benzoic acid period the phosphoric acid increases a trifle (about 3 per cent.) in the feces and decreases by about the same amount in the urine; in the after period the increase in the feces and the decrease in the urine were somewhat more marked. Wiley draws no conclusions from this. This might be considered as a poorer utilization of the phosphorus of the food, but greater guarantee as to the amount and nature of combination of the phosphorus compounds given would be necessary. That the number of blood corpuscles would also show variations was clear. Their number was greatly raised, as compared with the fore period, after 2 administrations of benzoic acid; considerably decreased, as compared with the fore period, after 3 administrations; was about the same as in the fore period after 1 administration; increased after 3 doses of sodium benzoate; decreased after 2 doses; not determined after 1 dose.

The general average for all twelve persons was as follows:

	Red Corpuscles	White Corpuscles	Hemoglobin Per Cent.
Fore period	5,082,273	7,433	96
Benzoic acid period	5,099,583	7,331	96
After period	5,255,000	6,644	97

No conclusion, therefore, for or against benzoic acid can be drawn from these data, yet Wiley speaks of a "tendency" to diminish the red corpuscles which benzoic acid seems to have!

In my honest opinion, Wiley's publication is lacking in every proof, every objective proof, of the harmfulness of benzoic acid, and from the voluminous metabolism investigations which Wiley has so pitilessly used against benzoic acid exactly the opposite conclusion may be drawn, that, at most, each and every entirely subordinate fact (I have carefully reproduced them all above from Wiley's publication) remains somewhat doubtful in its significance. From Wiley's report on subjective symptoms of digestive disturbances by 1–2 gram quantities of benzoic acid in wafers, no conclusions can be drawn; even with this method of procedure, so ill-adapted to our problem, the data stand too isolated in the literature. They give the impression that suggestion played a considerable part in them, for there is no doubt that in Wiley's laboratory preservatives are considered with extreme mistrust and antipathy and that the test persons should imbibe some of this antipathy is easily conceivable. I surmise that this was also the general impression of the American authorities and that new investigations by Chittenden, Long and Herter were instituted to critically test Wiley's results, which were contrary to all observations so far made in the laboratory and in practise.

Although the volume of analyses and observations reported by Wiley and their evident cost are considerable, the extent of the work of the other investigators is surprising. This voluminousness of the work, especially the numberless tables, makes it difficult to enter very much into the details of it. Moreover, it is really a question of three independent pieces of work. The three gentlemen, entirely independently of each other but following a carefully preconcerted plan, each subjected 6, 6 and 4 young physicians and chemists, respectively, to metabolism investi-

gations lasting not less than four months. There were thus $16 \times 120 = 1{,}920$ test days. On 40 of the 120 days, *i. e.*, on 640 days, exact urinary analyses were made. The benzoic acid was given in the following manner: In a fore period of 10 days no benzoic acid was given, then for two months 0.3 gram was given daily, then none for 5 days, 0.6 gram for 7 days, 1 gram for 7 days, 2 grams for 7 days, 4, 6 and 10 grams, respectively, for 7 days, and finally for 10 days no benzoic acid was given (the individual experimenters worked a little differently); the benzoic acid was mixed with the food and given as sodium benzoate. It must also be pointed out that the test persons were not exclusively of especially robust health; in fact, some of rather moderate constitution were taken.

In the first pages of the work is given, briefly, clearly and concisely, what interests us most, the conclusions of the three investigators.

Sodium benzoate in small doses, less than 0.5 gram mixed with the food, is without toxic effect and does not disturb health. Sodium benzoate in large doses, up to 4 grams mixed with the food, showed, upon investigation, no harmful effect in the general sense of the word. In some directions were observed small changes in certain physiological processes, the exact significance of which changes is not known.

According to the investigations, the mixing of sodium benzoate in smaller or larger doses has no unfavorable or deteriorating influence on the value of such food. In this series of experiments, also, the test persons were not in exact nitrogen equilibrium. They were allowed to enjoy at will, within certain limits, quite complicated, extremely varied and dainty foods, the food being merely weighed and analyzed. The daily consumption of food, which, in Germany in such experiments, we keep as nearly as possible uniform in nature and equal in amount, varied not inconsiderably, even though it must be confessed that the daily consumption of food by this method was relatively uniform. We can, therefore, without more evidence, just as easily draw favorable conclusions for benzoic acid from the slight increases in weight which almost all of these well-fed test persons showed as unfavorable ones from the opposite facts in the Wiley experiments. It only shows that benzoic acid is not grossly harmful. Exact metabolism observations in the absolute sense are, also, impossible; only relative values can be discussed exactly, and here, too, the so widely varying nutrition is a somewhat disturbing factor.

The utilization of dry substance, fat and albumin, as we are accustomed to observe it in Germany, is not ascertained here. But I have convinced myself, from a series of values which I have calculated, that no influences on the utilization are to be observed. The corpuscular elements in the urine were not, contrary to the observations of Wiley, increased. No general symptoms were noted, either with the smaller or the larger doses. Of the numberless details of the three investigations we can safely say that, like Wiley's, they show that in almost all special cases nothing essential is changed by the administration of benzoic acid; I may therefore confine myself to picking out a few points. Chittenden expresses, at the end, his extraordinarily favorable opinion that sodium benzoate, up to a dose of 4 grams daily, is no more disturbing or harmful to the human organism than the same quantities of salt. He makes no reservation in any direction, draws no suspicious conclusions from his observations, not even from the establishment, by Wiley's results, that the excretion of benzoic acid as hippuric acid in the urine is somewhat retarded. Long obtains very favorable results; he did, indeed, observe in his uneducated test persons (institute help, etc.) a slight disturbance at times (headache, vomiting, disturbance of sight, excitement) but always found a plausible explanation for it and never feels forced to ascribe it to benzoic acid. Herter noticed, especially with large doses, a slight increase of indican in the urine, but no change in the alkylsulphuric acid, a decrease of the fecal bacteria which evolve gases and an increase of the cocci as compared with other bacteria in

the feces. All these facts find a natural explanation in a somewhat decreased carbohydrate fermentation in the feces and somewhat increased scission of albumen, owing to certain bacteria or bacterial functions being favored by the benzoic acid at the expense of others; no one will interpret this as being hygienically dubious. Herter's values (p. 747) show very prettily the smooth transformation of benzoic acid into hippuric acid; according to his experiments, there is no appreciable retardation. A striking fact in all four of Herter's test persons was the increase of free hydrochloric acid in the expressed gastric juice during large benzoate doses, which could easily be considered as a symptom of irritation of the stomach, but is also capable of other explanations. Gerlach's experiments (see below) showed nothing of this in man or animal. There are many other details in this voluminous book, but they are hardly of significance for the problem and I have tried to bring out anything that might be disadvantageous for benzoic acid.

Most perspicuous to us Germans is the investigation carried out by Dr. Gerlach, of Wiesbaden, by German physiologico-pharmaceutical methods and which, though far behind the American works in scope and extent, can be the more easily surveyed as a whole. Small, medium and large doses were tested on animals and men. I shall not enter into the details of the experiments with rabbits, which were able to take very large doses of benzoic acid without any effects. One gram of benzoic acid was borne subcutaneously by a rabbit for 12 days without harm; a dog was fed with 7.5 grams of sodium benzoate in one dose without disturbing effects; in experiments on the investigator himself, 10 grams of benzoic acid, taken within 3 hours, had no influence on the pulse, respiration, body temperature, digestion and general well-being. (Not every stomach can stand such a dose!—Abstractor.) Two persons daily took 0.5 and 1 gram of benzoic acid and sodium benzoate, respectively, for 80–90 days without any effect. The most careful observation showed no special disturbances of the respiration, pulse or body temperature after taking 1 and 2 grams of benzoic acid and sodium benzoate, respectively. The action of the digestive ferments, gastric juice, trypsin and the diastatic ferments was not influenced by benzoic acid and sodium benzoate. Three and five grams of benzoic acid and sodium benzoate had no influence on the quantity, acid content and digestive power of the gastric juice of a dog subjected to the Pawlow operation. Half a gram and one gram of benzoic acid and sodium benzoate, respectively, taken by the experimenter at a test breakfast, exerted no influence on the free hydrochloric acid, total acidity and digestive power of the gastric juice, collected one hour after the breakfast. In metabolism experiments on men, it was found that 1 gram benzoic acid or 1.5 grams sodium benzoate (each taken 6 successive days) had no effect whatever on the stock of albumin in the body nor on the utilization of the nitrogenous substance and fat in the food. Eighty-two doses of 1 gram each of benzoic acid, taken within 86 days, and 88 doses, taken within 92 days, showed no unfavorable effects on the general well-being, body weight, etc. At the end of the experiments, it was found that the benzoic acid, whether mixed foods or chiefly vegetables were eaten, was completely transformed into hippuric acid. Gerlach collects from the literature a large number of cases, not all of which are contained in my earlier review, and which show that large doses of benzoic acid can be taken by healthy and by sick people without harm, and he concludes with the words:

> Neither the observations made in medical practise and reported in the literature nor the large number of experiments which have been carried out for the purpose seem to justify the assertion that benzoic acid and sodium benzoate in small quantities, such as they are eaten in preserved foods, are able to produce any harm whatever. The attempt to prohibit the use of benzoic acid and benzoates as a preservative would therefore not be warranted by the very copious scientific material at hand.

While the American investigators and Gerlach thus reach conclusions which could not be more favorable to those who espouse the cause of benzoic acid, the question as to the admissibility of its use is not yet wholly cleared up in Europe, especially in Germany, and an unfriendly disposition predominates. France summarily prohibited the use of benzoic acid as early as 1888, long, therefore, before its thorough investigation; in Austria, also, it was excluded from use by a decision of the chief of the Board of Health on the 16th of December, 1899, and recently again he has refused his sanction.[5] The Landesmedizinalkollegium of Saxony has likewise refused to authorize its use.[6]

In Germany the law takes no firm stand against benzoic acid. The acquittal of dealers who introduced margarine preserved with benzoic acid into the trade in Saxony is interesting. The produce dealer Gertrud Eberlein, née Herber, who had been fined 2 Marks for dealing in margarine with 0.05 per cent. benzoic acid, had started a long trial in three courts which, however, had ended with her acquittal in the assessors's, district and provincial courts. Injury to health by such doses of benzoic acid no one could maintain; the opponents of benzoic acid among the judges asserted that benzoic acid is really a preservative, that the addition of benzoic acid makes it possible to store margarine, but that the public demands fresh margarine and that the stored product is not as good as the fresh margarine. The arguments frequently made by the judges in favor of acquittal were the following: Benzoic acid has thus far not been prohibited in Germany; it occurs in cowberries in considerable quantities; in the amounts in question it is harmless; the nutritive value, wholesomeness and usefulness of the margarine is not changed; it is therefore a preservative which, where applicable, offers great advantages long sought for, without any disadvantages. The following judicial verdicts concerning benzoic acid in margarine have come to my knowledge:

(1) Acquittal of the manufacturer F. A. Isserstedt, Elberfeld, before the assessor's court of Solingen on November 11, 1908.[7] (2) Acquittal of the manufacturer Held, Schkeuditz, before the assessor's court of Schkeuditz.[8] (3) Acquittal of the same before the provincial court of Halle. Appellate proceedings on May 3, 1910.[9] (4) Acquittal of the merchant G. Eberlein before the assessor's court of Dresden in June, 1910.[10] (5) Acquittal of the same before the district court of Dresden on February 8, 1911.[11] (6) Acquittal of the same before the provincial court at Dresden on June 7, 1911.[12]

The following express themselves in favor of the use of benzoic acid (for margarine): (1) Professor Eccles in his book, "Die Bedeutung der Konservierungsmittel für die menschliche Ernährung in wirtschaftlicher und hygienischer Hinsicht"; (2) von Vietinghoff-Scheel;[13] (3) Gustav Hefter;[14] (4) the food chemist, Dr. Langfurth, Altona;[15] (5) the food chemist, Dr. Postler, Mühlhausen;[15] (6) Professor Klostermann, Halle; (7) the food chemist, Dr. Niederhäuser, Wiesbaden;[16] (8) the food chemist, Dr. Lührig, Breslau;[16] Professor Frerichs, Bonn.

Recently the Prussian Committee on Medical Affairs has expressed itself, in a decidedly surprising manner, in favor of the prohibition of benzoic acid as a preservative for human foodstuffs and luxuries. The referees, Privy Councillors Heffter and Abel,[17] frankly acknowledge, indeed, the conclusiveness of the

[5] *Das österr. Sanitätsw.*, 1910, Nos. 13–15.

[6] *Jahresb. Medizinalw. Königsreichs Sachsen*, 1905, 193.

[7] *Z. Marg.-Ind.*, 1903, 69.

[8] *Ibid.*, 1910, 113.

[9] *Z. Marg.-Ind.*, 1910, 113.

[10] *Ibid.*, 1910, 152.

[11] *Ibid.*, 1911, 69.

[12] *Ibid.*, 1911, 159.

[13] *Chem. Ztg.*, 1909, 181; 1910, 904.

[14] "Technologie der Fette und Oele," Vol. 3, p. 192.

[15] *Z. Marg.-Ind.*, 1910, 152.

[16] *Ibid.*, 1911, 69; cf. also *Chem. Ztg.*, 1911, 28, 53.

[17] "Die Verwendung von Benzoesäure und ihren Salzen zur Konservierung von Nahrungsmitteln," *Vierteljahrschrift ger. Med. u. öff. Sanitätswesen*, 41, 330 (1911).

proof afforded by the investigations of Chittenden, Long and Herter that small amounts up to 0.5 gram are harmless to the human body, but they continue:

> Whether larger amounts (quantities of several grams) could, in the long run, be borne equally without effect by everybody can not as yet be answered with certainty. The experiments of the American scientists in this connection are of too limited extent and suffer from certain limitations so that they can not be considered as proof of universal harmlessness.

With these carefully worded sentences we can, on the whole, agree without, however, drawing the conclusion therefrom that benzoic acid as a preservative is to be prohibited. For, first of all, we must consider that there are many food constituents which, under such a system, we might mistrust and likewise prohibit. I might mention hops in beer, sugar in preserves and salt in salt meat. There are, beyond doubt, many of whom it can be said with certainty that not only the repeated consumption of such things, but even a single meal off of them, is prejudicial to their health. Those suffering from urethral diseases can not stand hops, diabetes patients and persons with stomach troubles can not stand preserves, kidney sufferers can not stand salt, yet such sufferers are continually eating these things, consciously or unconsciously, without any one prohibiting them to the world. Secondly, from what has been said above it is evident that benzoic acid, considering its preservative action on acid substances poor in albumin (for others it does not come into consideration), is remarkably harmless to the healthy and the sick—with no other preservative have such daring experiments been performed on sick persons without harm. If a healthy or a sick person were given just once ten times the amount of tobacco, alcohol or even of sugar that is usually taken rationally, the results would be far worse than when, instead of 0.1 to 0.5 gram benzoic acid, 1–5 grams are given. I offered this same argument, with equal justification, many years ago to those who persisted in considering saccharin as a poison.

Thirdly, no one has thus far asked more than that it be considered whether certain special food products might not, under proper declaration, be treated with small quantities of benzoic acid. No one, except perhaps a few manufacturers, has ever dreamed simply of a permission to add benzoic acid to foods. On the contrary, there can not be much doubt that for meat and meat preparations benzoic acid and benzoates should be prohibited; they have little preservative power and when mixed with other substances (phosphates) they help to preserve a fresh color, while the decomposition goes on; like the sulphites, therefore, they give a deceptive appearance of better condition, and according to the Heffter-Abel decision even a slight odor of putrefaction of minced meat is masked or removed by the admixture of 0.25 per cent. of benzoic acid or benzoates. No one will demand the free use of benzoic acid for tinned vegetable preserves; in these cases nothing but heat is necessary. The question is whether its use for preserving egg yolks, tomatoes, lemon juice and similar preparations, as well as margarine (and this is at present the most important), can be permitted. The first-named preparations, egg yolks, tomatoes, lemon juice, are of subordinate interest as regards public nutrition and can be passed without further discussion; even the most timid would not need to fear gravely for the public health if benzoic acid in such preparations were allowed, a statement as to the amount used being required. Personally, I am unconditionally in its favor when it is proved that these products can be prepared in a non-perishable form and kept without preservatives only with great difficulty or not at all.

The question as to allowing the use of benzoic acid in margarine, which has become a real public foodstuff, demands special treatment. One hundred and fifty million kilograms of margarine are said to be produced annually in Germany and a considerable part of it used in this country.[18] It would be very difficult today, especially for those living in cities, to protect themselves against margarine in their food. Any one in a hotel or purchasing fatty

[18] *Z. Marg.-Ind.*, 1910, 134.

confectionery or bakery wares will at once make the acquaintance of margarine; only the preparation of all food in one's own house insures safety. Margarine has won this position in spite of all compulsory declarations, in spite of the foreign-sounding name, because it fills the need of the people for a cheap fat. Leaving out one recent case where the criminal negligence of a firm in carelessly selling a poisonous foreign fat caused many cases of illness and some deaths, so far there has not been much with which margarine can be reproached.

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WÜRZBURG

SCIENTIFIC BOOKS

Planktonkunde. Von DR. ADOLF STEUER. Leipzig und Berlin, Teubner. 1910. Pp. xv + 723. Mit 365 Abbildungen im Text und 1 Tafel.

Leitfaden der Planktonkunde. Von DR. ADOLF STEUER. Leipzig und Berlin, Teubner. Pp. 382. Mit 279 Abbildungen im Text und 1 Tafel. M. 7. Geb. M. 8.

Many additions have been made to our knowledge of the floating life of the sea and its counterpart in fresh water, the plankton, since Haeckel published his highly theoretical "Plankton-Studien" with its elaborate but never generally adopted nomenclature and classification of this domain of life. The work of the Kiel school and its Plankton Expedition and Henson's Danaid task of taking a census of the sea, the *Valdivia* expedition with its superbly illustrated reports, the various expeditions of the Prince of Monaco, of the U. S. Steamer *Albatross* under the direction of the late Alexander Agassiz, the work of the International Commission for the Investigation of the Sea and the investigations of the fresh-water stations in Denmark, Germany, Switzerland and the United States have resulted in the perfection of instruments and methods and the accumulation of a mass of results. Steuer's treatise on planktology thus finds the time opportune for appearance and fittingly forms a volume in the Teubner series ("Naturwissenschaft und Technik in Lehre und Forschung") under the editorship of Professors Doflein and Fischer, of Munich.

The first work is comprehensive in plan, covering all phases of the varied content of the subject of the life of the sea and of fresh water. The first chapter of the work deals with water, its distribution on the earth, chemical constituents, temperature changes, its relations to light and pressure, its color and odor, and its movements under meterological influences. Other chapters treat of methods, quantitative, qualitative and statistical, of plankton investigations, of the adaptations of the plankton to flotation with special reference to viscosity in relation to temperature and season, of adaptations in color, and of phosphorescence.

It appears that the data cited by Steuer are quite inadequate to establish his thesis that the organisms of the plankton as compared with those of the bottom and shore are characterized by a relatively low rate of reproduction. It is not the pelagic nature of the organism which is the cause of the low rate of reproduction often observed in plankton-poor lakes but rather a poverty stricken habitat. The herring fisheries, the chalk beds of past ages, the abundant and rapidly fluctuating plankton of enriched rivers, the occasional sudden outbursts of the "mare sporco," all bear indisputable evidence of the capacity of pelagic life to respond to opportunities for rapid multiplication. This erroneous idea that the plankton has a relatively slight capacity for reproduction is correlated with another all too widely applied idea, namely, that the tropical seas are relatively barren. The facts are that fresh waters and the sea vary greatly in different regions and at different seasons in the amount of life they contain. In warm waters the chemical processes of life are so accelerated that life cycles are shortened and decay is hastened, while in colder waters growth and decay are slower and individuals accumulate though the total product in the two regions in a given time may be the same. Food supply and temperature affect

the rate and amount of reproduction in water as they do upon land.

The question of the biological stratification of the haliplankton is treated at length. Investigation has shown that the marine phytoplankton is mainly limited to a relatively thin superficial region about 200 m. in thickness and that it ceases to grow below 400 m. The zooplankton on the other hand descends, in the opinion of many investigators, to greater depths, feeding upon the débris of the phytoplankton and upon cadavers, descending from the more densely populated zone above. The *Valdivia*, which made but few hauls at great distances from continental influence or away from great currents, found evidence in closing net catches of animal life extending to great depths.

Steuer's presentation of Agassiz's results and conclusions in this contested matter is far from adequate. He designates the Pacific as "sehr planktonarme" and "wenig günstig" for the examination of this question of the extent of life below 400 fathoms. The plankton of the Pacific is certainly as rich in large areas and probably no poorer in others than that of other oceans, while the oceanic conditions it presents for investigation could hardly be more typical or more favorable for the determination of this question. Nor is Steuer's statement that Agassiz's admission of the existence of a true bathybic plankton is to be accepted as placing him in accord with his opponents, to be regarded as either adequate or critical, for Agassiz limited this bathybic plankton to regions of continental influence and to territories below richly laden currents. Furthermore, to characterize Agassiz's discoveries of so-called deep sea animals, such as *Pelagothuria* and fishes as "gelegentliche Auftretungen" of the bathybic fauna in the upper levels of the sea is to dismiss a mass of evidence with a wave of the hand.

The horizontal divisions and the geographical and seasonal distribution of the plankton are discussed at length and its relation to the economy of nature and to man are analyzed. Under the latter caption attention is called to the relation of those occasional outbursts of species resulting in discolored seas and disturbances in the balance of nature, to plankton as a source of food for man and for fishes, to pond culture, to purification of polluted streams, to fish culture, to plankton as an index of productive capacity of water, and to the relations of plankton organisms to education, research and the fine arts.

A compiled work of this sort, the first in its field, is necessarily incomplete and imperfect. Noticeable omissions in both works are the Lucas sounding machine, Whipple's thermophone, Nathanson's discussion of the relation of vertical circulation to the problem of maintenance of fertility of the sea, Petterssen's, Ekman's and Nansen's contributions to facts and theories of oceanic circulation, Bauer's work on vertical migrations, and, in general, an occasional lack of perspective in dealing with the results of Scandinavian, English, French and American literature of the subject—defects easily remedied in a new edition. Dr. Steuer has done a great service in putting in so concise and compact a form the most striking and many noteworthy achievements in the field of planktology. The illustrations are numerous, usually good and well-executed. Exception may be taken to inverted figures of *Ceratium* (p. 476) and to an inverted figure from Okamura of a crushed Peridinian labelled *Gonyaulax polygramma*. Much better figures of this species have long been available.

The second work is an abridgment of the first, omitting some of the illustrations, the extensive bibliographies and some of the more technical treatment of the chemistry and physics of water and some of the details of the discussion of the seasonal distribution of the plankton.

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The Realm of Ends or Pluralism and Theism: The Gifford Lectures Delivered in the University of St. Andrews in the Years 1907–1910. By James Ward, Sc.D. (Camb.), etc., Fellow of the British Academy and of the New York Academy of Sciences, Professor

of Mental Philosophy, Cambridge. New York, G. P. Putnam's Sons. 1911. 8vo. Pp. xv + 490. Price, $3.00 net.

It was inevitable that the Gifford foundation for "natural theology" in the four Scots universities should produce some notable books, one or two bad books, and non-significant books not a few. Professor Ward's Aberdeen lectures of 1896–98, published under the title, "Naturalism and Agnosticism," have taken rank in the first class long since, as three editions attest. They embody a criticism of the Spencerian type of thinking, so astute and resourceful that, possibly, the assault need never be undertaken again. The present lectures are no less important, but, unlike their predecessors, they travel far from the beaten track of the "positive" sciences. It is the more necessary, then, to describe their contents fairly for readers of SCIENCE.

In the decade that has elapsed between the two courses, the fortunes of the "mechanical theory" have tended to sink lower and lower, so much so, that the spirit of revolt against it may be said to dominate contemporary philosophy, particularly in France, and this with scarce a protest, certainly with no effective protest, from the scientific camp; nay, if anything, with approval—witness McDougall's recent "Body and Mind." But, if the "mechanical theory" be thus relegated to the limbo of discards, we are confronted forthwith by a new situation. We must try "to ascertain what we can know, or reasonably believe, concerning the constitution of the world, interpreted throughout and strictly in terms of Mind" (p. v). Professor Ward's new work is a candid attempt to face this problem, and to think it through. Accordingly, its plan and main contents are as follows:

Part I. is a most adequate discussion of pluralism. After two introductory lectures (I., II.) and a discourse on the fundamental nature and contemporary ascendancy of this theory (III.), the author analyzes contingency in the world (IV.), evolution (V.) and the pluralistic goal (VI.). He then devotes two lectures (VII., VIII.) to Hegel—"it must be confessed, largely a digression" (p. vi). Part I. ends with critical treatment of the limits and difficulties of pluralism (IX., X.), whence Part II., Theism, originates. "For pluralism, though empirically warranted, we find defective and unsatisfactory; but the theism to which it points is only an ideal—an ideal, however, that, as both theoretically and practically rational, may claim our faith though it transcend our knowledge" (pp. v, vi). Here Professor Ward plunges *in medias res* at once, with discussion of the idea of creation (XI.), and of the cosmology of theism (XII.). Investigations of freedom and foreknowledge (XIII., XIV.) follow. Lectures XV.–XVII. deal with the problem of evil in its relations to pessimism, optimism and the moral order. Lecture XVIII. presents theories of a future life and, as a pendant, we have a chapter on faith and knowledge (XIX). The concluding lecture, the Realm of Ends, is in the nature of a summary, and may be read as an introduction to the book. Twenty-seven pages of supplementary notes (on contingency, creation, the relation of body and mind, the temporal and the eternal, and the divine experience), and an adequate index, complete the volume.

In view of the subjects discussed, it is well, perhaps, to add by way of elucidation, if not warning, that Professor Ward has always kept in view the condition laid down by the founder. Lord Gifford's will contains this direction:

> I wish the lecturers to treat their subject as a strictly natural science, . . . without reference to or reliance upon any supposed special or exceptional or so-called miraculous revelation. . . . They may freely discuss all questions about man's conceptions of God or the Infinite, their origin, nature, and truth, whether he can have any such conceptions, whether God is under any or what limitations, and so on.

This is precisely what Professor Ward has done; and, to quote the words of the first lecturer on the foundation, Max Müller, the book "must be reckoned among the signs of the times, pregnant with meaning." For it is the latest pronouncement, not simply of a veteran, but of one of the ripest and, from the scientific point of view, best equipped, among living thinkers.

Taking the work as a whole—for space forbids detail—the following remarks and excerpts may help to indicate its attitude and quality. Dr. Ward adopts the pluralistic position in pragmatic fashion. He insists that, as a matter of fact, we do start from a plurality of existing things. On the other hand, he nowhere affirms these things to be self-subsisting and, to this extent, leaves a convenient loophole. The most interesting result, from the scientific standpoint, is, possibly, the interpretation of evolution. "It is often applied to processes that are diametrically opposed, to the differentiation of a unity and to the integration of a plurality (97). . . . To the supposed unfolding of an organism regarded as completely pre-existing in miniature within the germ. . . . The successive unfolding of such a system, . . . though the *ne plus ultra* of evolution literally understood is then the direct negative of evolution as we understand it to-day. According to this later theory each new organism is not an 'educt,' but a 'product,' to use Kantian phrases: its *parts* are in no sense present in the embryo, but are gradually organized, one after another, in due order, as the term epigenesis implies and as Harvey, who first used the term, prophetically maintained (98). . . . It is the parts, the many, with which the pluralist starts: the question, whether or no there is an absolute whole prior to—at once the logical and the real ground of—all the parts, is for him not the first question but the last" (101). Plainly, the pluralist is tied to epigenesis: "The life-history of the race is original, is a long process of gradual acquisition by way of trial and error, in short, answers to what we have identified with *natura naturans;* whereas the genetic history of the individual is a derivative, rapid and, so to say, substantially invariable process, in a word, is routine or *natura naturata*" (207). Yet, even so, pluralism has its own difficulties. An "absolute plurality" "would be merely a sporadic manifold of realms of ends having a common physical basis but devoid of all teleological continuity (185). . . . A Supreme Spirit confronted and conditioned by free agents certainly does not correspond to the notion usually entertained of the Deity. Such a 'finite God' many would disown as a manifest contradiction in terms; yet beyond this it does not seem possible for the pluralist to go" (194). Nevertheless, pluralists would deny that evolution requires a transcendent Prime Mover distinct from the Many: for the Many they hold are all prime movers, and so far *causæ sui*. . . . The efficient causation in the world then is just this totality of prime movers, its final causation their organization into a higher unity" (199). That is to say, singularism, with its Absolute, is not the sole alternative; nor is scepticism the consequent recourse; therefore the contradictions as between the One and the Many remain to be overcome. The lectures on Theism offer the solution, or considerations towards it.

Theism "introduces one essential modification, at any rate, viz., the idea of creation. It does not, that is to say, assume merely that one transcendent Being exists above and beyond the whole series of the Many, however extended; but it assumes further that this one Being is related to them in a way in which none of them is related to the rest: they do not simply coexist along with it, they exist somehow in it, and through it" (231). This implies the difficulty, admitted to be insuperable (245), of the self-limitation of Deity. Hence, turning again to experience, "between the intemporal world of ideas and the temporal world of phenomena, free agents have their place" (306). Accordingly, the ideal realm of ends is "the achievement of virtuous struggle crowned eventually with victory" (374). Consequently, as to immortality and faith, "death becomes indeed but a longer sleep dividing life from life as sleep divides day from day; and as there is progress from day to day so too there may be from life to life (407). . . . Hence the moral ideal, as it leads to faith in God, leads also to the belief that the spirit world has other dimensions than those of the time and space that encompass the world of phenomena. . . . Has not God been mocked and life called the vanity of

vanities on the assumption that the present world is all?" (429).

Despite my radical dissent from Dr. Ward's pluralism, with its concessions to what I am bound to call irrationalism, his book has made a profound impression upon me. It is a work which any man may well peruse as a discipline in self-education, and this without reference to the field of his specialty. Assuredly, we have to thank Dr. Ward for a human, and therefore significant—often a wise—pronouncement. A main portion of the charm of the work is traceable to the skill shown by the author in conferring distinction upon the commonest things.

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BOTANICAL NOTES

THE PASSING OF THE SLIME MOULDS

DE BARY, whose keen botanical perception has perhaps never been equalled, long ago discarded the name Myxomycetes for the slime moulds, significantly applying to them the name Mycetozoa, and then placed them outside the limits of the vegetable kingdom, greatly to the consternation and indignation of many fungologists of the old school. The latter, relying upon external characters, associated them for a long time with the puffballs among the higher fungi, implying a relationship whose impossibility is now manifest to the merest tyro in botany. Latterly they have been pushed down into the neighborhood of the schizophytes (blue-green algæ, and the bacteria), as is done by Engler in his "Syllabus," but he takes particular pains to disclaim any relationship with the true fungi (*Eumycetes*), or higher plants.

More than thirty years ago the writer of this note said of them "they have no structural affinities with plants higher than they are, nor with any lower; they stand alone, and appear to belong to a different genetic line" ("Botany," p. 207), although in deference to the views as to their nature then prevalent they were still included among plants. By speaking of them as "a group of remarkable organisms which differ in many respects from all other vegetable structures," and by comparing them to certain Protozoa the attempt was made to educate the reader to regard them as aliens instead of true plants. In later publications they have been omitted as no longer necessary to be spoken of in a systematic arrangement of plants.

The final disappearance of these animals from the domain of botany seems now imminent, for in the new (eleventh) edition of the Encyclopaedia Britannica they are treated under De Bary's name of Mycetozoa, with the introductory defining phrase "in zoology, a group of organisms reproducing themselves by spores." The whole treatment, which is by J. J. Lister, is zoological, and no doubt is expressed as to their animal nature. We may now look for an approaching general revision of our botanical text-books so as to omit the Mycetozoa, greatly to the relief of the scientific botanists who have long been sorely puzzled to find a proper niche in which to fit them in the vegetable kingdom.

It will now be necessary for the zoologists to prepare to take charge of the considerable number of Mycetozoa to which they fall heir. It behooves the botanists to generously remove the specimens of these organisms from the pigeon-holes of their herbaria, and turn them over to the zoologists to be placed by them on the museum shelves devoted to the Sarcodinia among the Protozoa. And further it will become necessary for the librarians to revise their system of classifying botanical and zoological books so as to make the proper transfers upon their shelves and in their card catalogues. When all this is done the botanists may feel that they are well rid of these animals that have too long roamed quite too freely in the botanical garden. The "slime moulds" will have passed from the domain of botany, and there will remain in their stead only the "fungus animals."

THE SECRET OF THE BLUEBERRIES

IF to make "two blades of grass grow where but one grew before" has been regarded as a laudable undertaking, what shall we say of the successful effort to make blueberries grow

where they would not grow at all, and yet this is what F. V. Coville, the government botanist, seems to have done. In an interesting bulletin (193) of the Bureau of Plant Industry of the U. S. Department of Agriculture which appeared a little more than a year ago, Mr. Coville shows that blueberries (mainly of the species *Vaccinium corymbosum*, known as the swamp blueberry) differ from many ordinary plants in their soil requirements, and with the knowledge thus attained he has worked out a system of culture that promises to result in their successful culture. In the course of his paper it is shown that the swamp blueberry "does not thrive in a rich garden soil," nor on "heavily manured soil," and so for soil treated by lime, heavy clay soil, ordinary leaf mold soil, or any soil with a neutral or alkaline reaction. On the contrary, it is shown that it requires an acid soil, such as afforded by peat.

Applying these facts, Mr. Coville has successfully grown many plants of the swamp blueberry in pots, and their robust growth affords good promise of success in the field. The investigation with pot cultures is to be followed with those in the field and in this considerable progress has been made. "There is good prospect that the application of the knowledge thus gained [from the pot cultures] will establish the blueberry in field culture, and that ultimately improved varieties of these plants will be grown successfully on a commercial scale."

THE GRAMA GRASSES

David Griffiths has rendered a good service to students of the grasses by the publication of his paper on "The Grama Grasses" (Contrib. U. S. Nat'l Herbarium, Vol. 14, pt. 3, 1912), in which he describes the species (46) included in the genus *Bouteloua* (36), *Cathestecum* (4), *Pentarrhaphis* (2), and *Triaena* (1). By means of good figures the details of the spikelets are made plain, and these are supplemented by full descriptions, and a citation of specimens in the National Herbarium. The synonymy of these species is given very fully with critical notes. From this portion of the paper we learn why the more common species now bear the names *Bouteloua hirsuta*, *B. gracilis* (instead of *B. oligostachya*), and *B. curtipendula* (instead of *Atheropogon curtipendula*). One new species of *Cathestecum* and two of *Bouteloua* are described.

In speaking of their economic importance the author says: "It is doubtful whether there is another group of native pasture grasses which is of as much economic importance as this when both quality and quantity are considered." "The most promising of all the species for field cultivation is *B. curtipendula*, not that it grows any more readily than the others, but on account of its size and habit." "*Bouteloua gracilis* makes a splendid turf when sown thickly and well cared for." However, on account of the difficulty in collecting seed he has to say at last that "in short, it is more than probable that because of the lack of good seed habits in this genus, even the most valuable species can not become of importance in cultivation."

BOTANICAL NOTES

A number of papers on the fungi should be noted here—the first, by W. C. Coker and Louise Wilson (in *Mycologia*, November, 1911), on a curious "conjugating yeast" (*Schizosaccharomyces octosporus*) in fermenting grapes, in which the plants multiply vegetatively not by budding, as in other yeasts, but by fission. The "conjugating" cells constitute "a double sac that resembles a pair of saddle-bags" and in this structure eight spores are produced.

The "Experiments on Spore Germination and Infection in Certain Species of Oomycetes" (Research Bull. 15, Wisconsin Experiment Station) by I. E. Melhus, are concerned with the White Rust of Crucifers (*Cystopus (Albugo) candidus*). It was found that the spores germinate best in water and rather low temperatures, and that plants are more easily infected at low temperatures, also, probably "due to the increased percentage of spore germination."

DR. RICHARD DE ZEEUW publishes a helpful paper in the *Centralblatt für Bacteriologie, Parasitenkunde und Infektionskrankheiten*, 1911, on the "Comparative Viability of Seeds, Fungi and Bacteria when Subjected to Various Chemical Agents," in which he shows that the disinfection of seeds, etc., is by no means as easily accomplished as has been supposed, and that the results of many experiments requiring disinfection are open to criticism.

WE can not pass by without at least brief mention the *Journal of the College of Agriculture of the Imperial University of Tokyo* (Japan), in the December number of which are two articles by Professor Dr. S. Kusano, the first, on "*Gastrodia elata* and its Symbiotic Association with *Armillaria mellea*," showing that this chlorophyll-less orchid lives in a beneficially symbiotic relation with the mycelium of the fungus. The second paper, "On the Root-Cotton, a Fibrous Cork Tissue of a Tropical Plant" (*Fagara integrifolia*), shows that "the root-cotton is a kind of cork tissue derived from the cork cambium, which arises primarily from the pericycle or secondarily from the bast of the root." The author concludes that it is of economic value, "chiefly in its unwettable and less hygroscopic quality." The papers are illustrated by seven very fine plates.

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THE RELATION OF PIGMENTATION TO TEMPERATURE IN DEEP-SEA ANIMALS

PROFESSOR C. V. BURKE'S article on the relation of color of certain sea-animals to the depth at which they live[1] is of much more than ordinary importance, for it may be possible to coordinate these facts with similar ones as to tropical land animals which show that pigmentation is of great, if not vital importance in heat regulation. In nearly all the biological literature on the subject, it is assumed that the sole use of pigment is for concealment by more or less resemblance to something in the background or to the background itself. This may be true of all colors but pigment of any color, if opaque enough, may protect underlying tissues from death due to excessive light, a matter to which von Schmaedel first called attention as to man, nearly twenty years ago. This rule has now been found to be universal, for in every species there is a pigment or other protection proportionate to the intensity of the light. In the 1887 *Proceedings of the Royal Society of Edinburgh*, Dr. Robert Wallace, now professor of agriculture in the University of Edinburgh, published another epoch-making observation, which, like that of Mendel and of many others, was completely ignored for a quarter century. Wallace found that all the domestic mammals of the tropics had black skins, and though Huxley was much impressed by the universality of the phenomenon, he could suggest no reason for it because up to the time of his death very little was known as to the deadliness of the shorter ether waves to all naked living tissues, such as in the case of bacteria for instance; and though we then used sunlight to "disinfect," by killing our parasites, no one had yet perceived that it could also kill us. It is now known that the main purpose is light protection, and there is a wealth of evidence that if unpigmented stock is taken from dark climates to light ones it dies out. The agricultural experimenters have utterly failed to establish the big white swine in our west or in any light climate and at the present moment, in many parts of the world, farmers are vainly trying to breed imported stock insufficiently pigmented. The matter is of such great practical importance that it must be cleared up at once to stop the present wasteful methods.

It has also long been known that black assists heat radiation in all temperatures below body heat. These black-skinned domestic animals are then the fittest for tropical temperatures, but they can not expose themselves to the sun because of the fatal absorption of heat. They instinctively hide in the day, if the skin is not covered by a reflecting coat as in the Arab horse. In cold light countries

[1] SCIENCE, October 6, 1911.

excessive radiation is prevented by fur and hair, as in the black skinned Mongolian pony. The black skinned animals with little or no hair are therefore restricted to a very limited thermal range, while those with hair or fur protection are healthy in all extremes.

Some recent observations of horses in the Philippines, Japan and Korea have shown that slight differences in climate make great differences in hair color and it seems to depend more largely on air temperature than was formerly thought. The detailed data were published in a preliminary article in the September number of the *U. S. Cavalry Journal,* but the point of present interest is the relation of pigment to the environmental temperature.

Every kind of protoplasm has a very limited thermal range in which it functions best; in some cases there is practically no range at all for the function is disturbed by even a half degree of change. Every kind of protoplasm can live if subjected to higher or lower temperatures than its optimum even if it ceases functioning. But it is to be noted that no kind can stand very much rise of temperature, though it may be chilled to the absolute zero for a short while. The upper thermal death point is therefore so near the usual range and the malfunctioning due to any rise is so great, that each species adopts some safeguard against overheating. In the land mammals it is always some method of radiation or evaporation of perspiration and a dark color seems vital for the purpose of radiation in all temperatures below body heat. It is amazing to see the differences in horse colors in different localities according to temperature in Japan, Korea and wherever there has been no check to the process by importations for some centuries. Where there is heat to reflect at any season the color is light or white.

One of the puzzles of zoology has been to account for dense pigmentation where there is very little light. If there is complete darkness, as in some caves, the color of fishes is apt to disappear entirely by selection on the ground of economy, the pigment evidently being an expensive drain on the resources. As fish are said to have a temperature but little above that of the water in which they live, there is evidently little or no need for them to have safeguards against overheating or chilling in a medium, never hot enough to be injurious nor below 40° F. as a rule. Hence coloration of fishes has been invariably interpreted as for concealment, and the facts certainly do point to that reason.

In the deep sea, on the other hand, we have vastly different conditions. The only light is a phosphorescence which is usually so faint as to require very large eyes, yet some investigators assure us that it can be so intense as to demand dense pigmentation for concealment. This may account for the fact that the pigment is often spread all over the body, as would be necessary if the light comes from all directions and not always from above, as in the fishes of the upper layers. Yet there are curious exceptions, as though some deep sea places were as dark as caves, for some species have little or no pigment and some have no eyes; but as far as known, the relation between these facts, if there is any relation at all, has never been explained or proved.

With regard to deep sea temperatures we are assured that generally they are very low, specimens being too cold to handle. Salt water at such pressures can be much below 32° F. without freezing, and it is often found at a degree which prevents all activity of fish in surface waters. They "hibernate" in such chilly surroundings. There must then be a different law as to the relation of the body temperature of deep sea fish to the water surrounding them. All the mammals of the sea are densely pigmented and generally all over. They are never subjected to high temperatures nor to anything lower than 32°, and have no need for protection from extremes. The pigment then must be for light protection. They all prevent undue heat loss by the thick layer of insulating fat, so the radiation of heat by the black color can do no more harm than in the case of black cattle which are found in mild temperate climates. Black pigments can not be for the purpose of absorbing heat from the sun's rays, for all such animals avoid the

sun, nor can sea-mammals absorb to any extent that way as the heat does not penetrate enough. The surrounding medium is always below body temperature and the heat flow is always from the body not into it. As all mammals seem to depend upon more or less constant loss of superfluous heat by radiation and much loss during exertion, the black colors are probably of as great use to whales and porpoises, as to elephants and similar hairless tropical animals who likewise are never exposed to extremes and whose ranges of temperature do not differ markedly from those of the sea mammals. That is, there must be, as in horses, some means of preventing overheating, and color surely helps according as the animal is shaded or exposed to the sun. The amazing differences in horses can be logically explained on these lines, as seen in the above paper. Hence there is a suspicion that dark color in all sea-fishes at any depth has some unknown relation to the temperature, as well as the need of concealment. It must be remembered that the inner surfaces of shells, and the concealed surfaces of some parts of the bodies of many animals are most unaccountably pigmented. Neither the food nor protection from light nor resemblance to the background can be the reason, nor can it be a vestige of a previous useful condition. Indeed it is a puzzle which needs explanation, and it is requested that in all future studies there be facts presented as to the temperature of both the water and the body of the animal. It is not likely that the deep-sea fish can function at such low temperatures as we are told do exist, and it is probable that we shall find that where life is abundant the temperature of both fish and water is considerably above 40° but that in the vast areas where no life at all is found the cold may be the prohibitive factor. There must be as great variation from place to place by reason of currents as in the air. The prevalent winds make only a few degrees difference between the temperature of the northeast and southwest of Japan, but the horses in the first place are dark brown and in the latter light yellow for this reason alone. There must be similar local differences in the deep sea, due to currents to account most fully for differences in pigmentation. If the color darkens with increasing depth and coldness, then the animals surely lose by radiation what little heat they have and must be unable to function at all. Unless, then, there are different laws for the deep sea life, of which we can not conceive at present, there must be another reason for a concealing blackness which would be a fatal radiator to ancestral types nearer the surface. Indeed there may be some unknown laws of heat radiation under such tremendous pressures, which actually reverse matters and make the pigment a conserver of body heat in cold water. It can scarcely be believed that the color is to help absorption of heat, for there is little to be absorbed—the animal must depend on its own heat production. That is, the facts so far known do not explain the blackness of the deep sea fish.

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PHYSIOLOGICAL SEX DETERMINATION

AN interesting, if not convincing, collation of arguments in support of the notion that the suprarenal capsules are concerned in determining the sex of offspring was presented to the Paris Academy of Sciences on November 20, 1911, by Dr. R. Robinson. His arguments fall into three groups.

1. *Clinical observations.*

It has been shown by Dr. Fieux Agregi, of Bordeaux, that when the heart-beat of the fœtus is between 136 and 150 per minute there is born a female in 68 per cent. of the cases; if the heart-beat is more than 150 it is always a male. He had fifty cases. Robinson not only confirmed Agregi's observations, but was able to determine the retarding action of the administration of adrenalin upon the pulse of the fœtus. He therefore presumes that if this substance were administered from the early days of pregnancy it would influence the heart-beat and so the sex of the fœtus.

2. *Anatomo-physiological facts.*

The adrenal glands seem to influence the development of the individual after birth, if they do not cause the determination of the

sex immediately after conception; this is shown by their relations to some of the secondary sexual characters. In England a woman of 32 years who had never menstruated, had hair on face, etc., and other appearances of a man; an autopsy showed the presence of large tumor on the suprarenal. Bortz, the German surgeon, observed a girl of 16 who took on the characters of the male—beard, deep voice, distribution of hair on body, etc. Post-mortem examination showed tumor on suprarenal. A woman of 26 years was observed by a Dutch physician: she was obliged to shave like a man. Autopsy showed one of the suprarenals enclosed within the ovary. Similar facts have been published by many physicians.

3. *Chemical reactions.*

Is there any means, asks Robinson, of knowing approximately the functional state of the suprarenal capsules in the gravid female? This is worth studying if we are to come to an understanding of sex determination. Violent or obstinate vomiting and other symptoms of suprarenal insufficiency are not constant in their manifestations. He therefore used the Fränkel-Allers test (acid iodate of potassium) to reveal the slightest trace of adrenalin in the urine or blood. He used also the method of Vulpian (perchloride of iron); but found the per-iodate of potassium much more sensitive and certain in its reactions. By these means he foretold the sex of a fœtus in the fourth month.

From all these facts he thinks it is safe to conclude that the adrenalin influences the sex of the developing embryo, at least in the higher vertebrates or in man.

The hypothesis that adrenalin influences the heart-beat has been subjected to experiment and has been confirmed; but the hypothesis that the rate of the heart-beat determines the sex is one that may be tested experimentally, but has not yet been so tested. It is interesting to note that in the clinical observations given by Robinson the heart-beat of the male is higher than that of the female; this is contrary to the commonly accepted relation. The argument, moreover, may prove too much. If maleness is converted into femaleness by the adrenalin directly, or indirectly through the influence of this upon the circulatory system, it should be possible to bring on secondary female characters in a male by the administration of adrenalin before or at puberty.

The number of cases in which the sex was foretold on the basis of the Fränkel-Allers test for adrenalin in the blood or urine is not given by Robinson; presumably too few to warrant statistical conclusions. But even if the number were very large, and even if there should be found a constant relation between the sex of the fœtus and the quantity of adrenalin produced in the pregnant mother, these facts would by no means indicate a causal relation in the sense assumed by Robinson. It is quite conceivable that, in the case assumed, the physiological state of the fœtus determines the activity of the mother's adrenal capsules.

A more general criticism of Robinson's argument lies in the tacit assumption that sex differentiation means one thing in the higher vertebrates and man, and quite a different thing in the rest of the animal kingdom and in plants. It may well be that there are several distinct factors concerned in sex determination, but these must all belong to related categories. It is extremely improbable that sex is determined by an accessory chromosome among insects, by adrenalin among mammals, by traumatism among gramineæ, say, and by ultra-violet rays among mosses. It is interesting to note, in conclusion, that physicians have formulated several theories of sex determination in recent years, but always in complete innocence of the work being done by experimental biologists along this line.

Benj. C. Gruenberg
Commercial High School,
Brooklyn, N. Y.,
December 30, 1911

Societies and Academies

The American Mathematical Society

The one hundred and fifty-seventh regular meeting of the society was held at Columbia University on Saturday, February 24, extending through the

usual morning and afternoon sessions. The attendance included forty-one members. President H. B. Fine occupied the chair. The council announced the election of the following persons to membership in the society: Mr. J. W. Alexander, Princeton University; Mr. A. A. Bennett, Princeton University; Professor J. G. Coffin, College of the City of New York; Professor G. H. Cresse, Middlebury College; Mr. C. R. Dines, Dartmouth College; Professor H. E. Jordan, University of Kansas; Mr. F. S. Nowlan, Columbia University; Professor C. W. Watkeys, University of Rochester. Eight applications for membership in the society were received.

Announcement was made of the gift of Dr. Emory McClintock, second president of the society, of over four hundred valuable books to the library. The gift also includes a large number of pamphlets and reprints of important mathematical papers.

The following papers were read at this meeting:

S. A. Joffe: "Sums of like powers of natural numbers."

G. A. Miller: "Second note on groups generated by operators transforming each other into their inverses."

S. Lefschetz: "On remarkable points of curves."

S. E. Urner: "Certain singularities of point transformations in space of three dimensions."

A. B. Coble: "The characteristic theory of the odd and even theta functions as related to finite geometry."

H. H. Mitchell: "Some quaternary groups with particular prime moduli."

J. E. Rowe: "The undulation and cusp invariants of the R^n."

W. F. Osgood: "A necessary and sufficient condition that a single-valued function in a projective space be rational."

Dunham Jackson: "On the convergence of the development of a continuous function according to Legendre's polynomials."

K. P. Williams: "The solution of non-homogeneous linear difference equations and their asymptotic forms."

E. J. Miles: "Note on the isoperimetric problem with discontinuous integrand."

Dunham Jackson: "On approximation by trigonometric sums and polynomials."

J. E. Hodgson: "Orthocentric properties of the plane directed n-line."

The date of the next regular meeting of the society falls on April 27. The Chicago Section and the San Francisco Section will both meet on April 6.

F. N. COLE,
Secretary

THE AMERICAN CHEMICAL SOCIETY
NEW YORK SECTION

AT the meeting held at Rumford Hall on March 8 the following sectional officers were elected to take office at the close of the present session in June: *Chairman,* Arthur B. Lamb; *Vice-chairman,* David Wesson; *Secretary-Treasurer,* C. M. Joyce; *Executive Committee,* A. C. Langmuir, G. A. Hulett, Allen Rogers and T. L. Briggs.

The Wm. H. Nichols medal (for the session of 1910–11) was awarded to Professor Charles James, of New Hampshire College, for his work on the rare earths.

Dr. W. Gilman Thompson read a paper on "Occupational Poisoning in Chemical Trades."

This paper awakened considerable interest and discussion, the meeting favoring further investigation of the subject and appointing a committee for this purpose.

Dr. A. M. Comey read a paper on "The Testing of High Explosives," illustrated by slides, and Mr. A. E. Marshall spoke on "Silica Ware: Its Manufacture, Properties and Uses."

C. M. JOYCE,
Secretary

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF BIOLOGY

AT the regular meeting of the Section of Biology, held at the American Museum of Natural History, December 11, 1911, Chairman Frederic A. Lucas presiding, Professor Henry E. Crampton gave a lecture, illustrated with lantern slides, on his recent explorations in Guiana and Brazil. Dr. W. K. Gregory concluded his communication on the "Origin of Paired Limbs," reviewing the rival claims of the Crossopterygii and Dipnoi for the ancestry of the Amphibia and reinterpreting the elements of the pectoral limb of the Upper Devonian Rhizodont genus *Sauripteris.*

At the regular monthly meeting of the section held at the American Museum of Natural History, January 15, 1912, the following papers were read:

Phylogeny and Ontogeny of the Horns of Mammals: HENRY FAIRFIELD OSBORN.

The recent discovery of the modes of origin of the horns in the titanotheres, a perissodactyl group remotely related to horses, tapirs and rhinoceroses, permits of a comparison of phylogenesis with the ontogenesis of the horns in bovine mammals. The

latter is based upon an osteological series recently prepared by Mr. S. H. Chubb, the former is based on the rich phylogenic series of Eocene titanotheres in the American Museum of Natural History. The conclusion is that ontogeny closely recapitulates phylogeny, that the genesis is gradual or continuous, that the horns arise definitely and determinately. In the bovine series it seems, in accord with the conclusions of Dürst, that the horn first appears as a circular thickening of the skin, accompanied by accelerated growth of the hair preparatory to the formation of the keratin of the horny substance, at a period considerably prior to any sign of the horn in the bony structure of the frontals. This raises the problem, which will form the subject of a special paper in the *Annals* of the Academy, as to what element first arises in connection with horn evolution, namely: (1) the psychic, or desire to use the horn, (2) the epidermal callous or keratin protection of the bony horn center, or (3) the bony or osseous horn itself. It would appear that the psychic tendency must precede the epidermal and that the latter precedes the osseous, but this disputed point requires further investigation.

Skull Measurements in Man and the Hoofed Mammals: Henry Fairfield Osborn.

Comparative anatomists and zoologists have been slow to introduce into mammalogy systems of measurement by indices and ratios, which have proved of such universal value in anthropology. It is found among the hoofed mammals, from studies undertaken by the author with the cooperation of Dr. W. K. Gregory, that *cephalic indices and limb ratios* between different segments of the skeleton are far more significant than systems of direct measurement. These cephalic indices of the gradual changes of proportion between different regions of the skull have the value of specific characters and sharply distinguish members of different phyla. For example in the cross between the horse (*E. caballus*) and the ass (*E. asinus*), it is found that the cephalic indices are transmitted as pure non-blending characters.

Among the most significant indices are the following: (1) the cephalic, which is obtained by dividing the total or basilar length of the skull by the zygomatic breadth; (2) the cranial, which is obtained by dividing the basilar length by the postorbital length of the skull; (3) the facial, obtained by dividing the basilar length by the preorbital length of the skull, etc. The horses show proopic dolichocephaly, or elongation of the face, and a static condition of the cranium, while the titanotheres, in contrast, show opisthopic dolichocephaly, or elongation of the cranium, and abbreviation of the face. Like the phyletic differences of proportion between the horse and the ass, these differences are most exactly expressed by the method of indices.

The application of the ratio method to the limbs of the hoofed mammals has again produced most surprising results. It is found that mammals of different phyla adapted either to "weight" or to "speed" converge respectively toward typical "weight" or "speed" ratios, which are obtained by dividing the length of the lower segments, tibia and radius, respectively, by the upper segments, femur and humerus, metacarpus and metatarsus, respectively. These "weight ratios" and "speed ratios" are far more significant as regards function and phyletic change than the actual or direct measurements.

This subject will be fully treated in the author's forthcoming monograph "The Titanotheres," to be published by the United States Geological Survey.

Whaling in the Olden Time: Frederic A. Lucas.

The speaker exhibited lantern slides illustrating some interesting pictures from old works on whaling and showing the methods practised by the early Japanese, European and American whalers.

At the regular monthly meeting of the section, held at the American Museum of Natural History, February 12, 1912, Professor Bashford Dean presiding, the following paper was read:

Some Factors of Geographical Distribution in South America: John D. Haseman.

The speaker outlined his itinerary in Central South America, where he had spent the years 1907–1910 collecting fishes, etc., for the Carnegie Museum and studying the geology, physiography and faunal complexes. He criticized the "static method" of studying geographic distribution as leading to no certain results. He outlined his conception of the geology and former topography of South America as bearing upon the problems of distribution and as being distinctly unfavorable to the "Gondwana land" hypothesis. He also analyzed the faunal similarities between South America and Africa and held that they offered no valid evidence for the former existence of a land bridge between those continents. His results are being published in the *Annals* of the Academy.

William K. Gregory,
Secretary